THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

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AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

VOL. XXII.—FIFTH SERIES.

JULY—DECEMBER 1886.

LONDON:

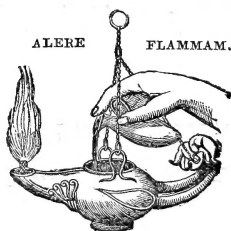TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

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"Meditationis est perscrutari occulta ; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

——"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

ALERE FLAMMAM.

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(FIFTH SERIES).

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NUMBER CXXXVII.—OCTOBER.

NUMBER CXXXVIII.—NOVEMBER.

NUMBER CXXXIX.—DECEMBER.

ERRATA.

PLATES.

I. *On the Variations of Gravity at certain Stations of the Indian Arc of the Meridian in Relation to their Bearing upon the Constitution of the Earth's Crust. By* Rev. O. Fisher, *M.A., F.G.S.**

IT is well known that, during the geodetic operations for the measurement of the Indian arc of the Meridian, it was found that the attraction of the Himalayas upon the plumb-line was less than it ought to have been, according to what Archdeacon Pratt calculated that their mass should have produced. This was attributed by Sir G. B. Airy to a downward protuberance of rock, having the same density as the mountains, into a substratum of greater density, so that their position would be approximately one of hydrostatic equilibrium ; and he showed that such an arrangement, by which their weight would be sustained, would have the effect upon the plumb-line of the kind, which had to be accounted for, greatly reducing the attraction of the mountains upon the plumb-line at a distant station, but much less so at a station near them †.

Pratt himself, however, preferred to attribute the anomaly to a supposed deficient contraction of the crust of the Earth beneath the mountains during the secular cooling of the globe, causing their relative elevation; and to this he attributed the diminution of density. He retained the average density

* Communicated by the Author.
† Trans. Royal Soc. vol. cxlv. p. 101 (1855), quoted in 'Physics of the Earth's Crust' (Macmillan, 1881), p. 145.

of the crust for the mountains themselves, and supposed an attenuation equal to their mass to affect the crust for a depth of 50 miles beneath them *.

Geologists, however, would hardly be satisfied with this explanation. For, although there are some areas which appear to have been vertically lifted within comparatively recent times, such as notably the Colorado Plateau, nevertheless the usual type of an elevated region is that of rocks which have been heaped together by lateral pressure, so that the material of which the mountains consist has been pushed horizontally towards the range over the nucleus ; and this is the case with the Himalayas. I have shown, in my ' Physics of the Earth's Crust,' that, if the substratum is plastic, this process would involve the production of the kind of downward protuberance that Airy postulates, and would explain the existence of a deficiency of the attraction towards the mountain-range. Accordingly, in the whole of my reasoning upon the subject, needing some working hypothesis, I have assumed that the crust is of the mean density of granitic rocks, viz. 2·68 ; and the substratum of the density of basic rocks, viz. 2·96. But I did not, in my book, make any quantitative estimate respecting the probable result of my assumptions upon the variation of gravity.

My attention was again drawn to this question by reading the instructive lecture delivered by General Walker, the Superintendent of the Trigonometrical Survey of India, at the meeting of the British Association at Aberdeen in 1885 †. This led me to look into the description of the pendulum experiments referred to in that lecture, which are published in vol. v. of the ' Account of the Operations of the Great Trigonometrical Survey of India '‡. If I understand the matter rightly, what was done was in principle this :—At certain stations of the Survey, of which the height and position had been already determined, the mean number of swings, called the " vibration-number," was observed, which were made by two pendulums in twenty-four hours which at the equator would have made about 86,000 § vibrations in the same interval. (It will be remembered that the number of seconds in twenty-four hours is 86,400.) In this manner the force of gravity at each station could be compared. The effect of local attraction at the station was then estimated, as well as

* ' Figure of the Earth,' 4th ed. pp. 201 and 208.
† ' Nature,' vol. xxii. p. 481 (1885).
‡ Prepared under the directions of Major-General J. T. Walker, C.B., R.E., F.R.S.   Calcutta, 1879.
§ Ibid. p. [129].

that of the height ; and when these together had been allowed for, the corrected vibration-number so determined was regarded as the vibration-number for that station reduced to the sea-level. Had these disturbing circumstances been correctly estimated, and had no others of a hidden kind existed, this vibration-number ought to have tallied exactly with that belonging to the latitude of the station. It turned out, however, that they were usually different, and for the most part in defect, often to a considerable extent. "There appears to be no escape from the conclusion that there is a more or less marked negative variation of gravity over the whole of the Indian continent, and that the magnitude of this variation is somehow connected with the height. Let us group the stations as follows, omitting Ismailia and Kew as foreign."*

"I. Coast and Island Stations.

Station.	Height in feet.	Apparent variation in vibration-numbers.
Punnæ	48	−2·65
Kudankolam ...	˙168	−2·56
Minicoy	6	+1·37
Alleppy	6	−1·21
Aden	5	−0·31
Mangalore......	7	−3·24
Madras	27	−3·39
Cocanada	9	−1·81
Colaba	35	+0·75
Calcutta........	18	−1·27
Mean ...	33	−1·43

" II. Inland Stations less than 1000 feet high.

Station.	Height.	Apparent variation in vibration-numbers.
Mallapatti	288	−3·77
Pachapaliam ...	971	−4·41
Usira	810	−3·60
Datairi	717	−4·30
Kaliana	810	−6·14
Nojli	879	−6·88
Meean Meer ...	706	−6·01
Mean ...	740	−5·02

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* *Loc. cit.* p. [142].

" III. Inland Stations 1000 to 2000 feet high.

Station.	Height.	Apparent varia-tion in vibration-numbers.
Namthabad ...	1173	−5·54
Kodangal	1914	−4·59
Damargida ...	1946	−6·56
Somtana.........	1714	−4·31
Badgaon	1120	−4·03
Ahmadpur ...	1693	−4·38
Kalianpur	1763	−3·61
Pahargarh......	1641	−5·60
Mean ...	1620	−4·83

" IV. Inland Stations over 2000 feet high.

Station.	Height.	Apparent varia-tion in vibration-numbers.
Bangalore	3064	− 5·68
Dehra	2242	−11·36
Mussoorie	6920	− 8·22
Moré	15408	−24·09

"

The method followed by Col. Herschel in estimating local attraction was to employ the formula for the attraction of a circular spherical cap, being "the part of a spherical shell bounded by a right cone, whose vertex is the centre of the sphere. Two such cones on a common axis intercept between them a zone, which is clearly the difference between two caps having a common axis."[*] The proper altitude of sections, every one of a set of such zones, taken around the station as their centre, each of appropriate width for its distance, was estimated, and also for the last zone the distance at which attraction would cease to be appreciable. The density of the masses was taken at 2·75, being half the mean density of the Earth. It is obvious that such an estimate would stop at the sea-level, and take no account of variations of density below it ; nor yet does it seem that the rise of the sea-level, which under the supposed conditions would be considerable, was allowed for. The for-mulæ used are essentially the same as those given by Pratt

[*] *Op. cit.* p. [151].

in the fourth edition of his 'Figure of the Earth,' to which work Col. Herschel acknowledges himself indebted [*].

I propose, in what follows, to compare the vibration-numbers at some of the meridian stations of the great Indian arc uncorrected for local attraction, but simply reduced to the sea-level [†], with what they ought to be there did no elevated tract exist ; and the result will show how much local attraction need be called upon to account for. Then, if a given hypothesis respecting the constitution of the Earth's crust fairly meets the requirements in several instances, it will afford a fairly strong presumption that the hypothesis has a foundation in reality.

It will be convenient to refer other stations to Punnæ as a base. It is situated near Cape Comorin, at the southern extremity of the peninsula, in latitude 8° 9' 28" N. But since Punnæ is 48 feet above the sea, it will be better to make the reduction, small though it be, to the sea-level there, once for all.

We shall employ the following symbols in the calculations, the numerical values being taken from the 'Account of the Pendulum Operations' already referred to.

c = the radius of the earth = 20,926,000 feet = 3963 miles.

ϵ = the elipticity, = 0·0034483.

m = ratio of centrifugal force to gravity at the equator, = 0·0034674.

h = height of the station.

l = latitude of the station.

l' = latitude of Punnæ.

N = number of vibrations at Punnæ in twenty-four hours when reduced to the sea-level.

$N + \delta N$ = the observed number at another station.

g = the Earth's attractive force at the equator.

G = the force of gravity at the station.

sG = the change in G corresponding to *one* vibration, where s = 0·0000023148.

k = the thickness of the cooled crust, taken as 25 miles.

ρ = the density of the crust, mountain, and its root, taken as 2·68.

σ = the density of the substratum, taken as 2·96.

h = the height of the station.

u = the chord of the semiarc of the spherical cap.

t = the depth of the root below the bottom of the crust.

w = the chord of the semiarc of the root of the mountain.

* *Op. cit.* p. [151].
† *Op. cit.* p. [120].

The formula for the time of vibration of a pendulum, $t = \pi \sqrt{\dfrac{L}{G}}$, enables us to connect N and G; and it is shown at p. [123] that, "as regards reduction for height of station, the formula $\delta N = \frac{1}{2} N \dfrac{\delta G}{G}$ is sufficiently exact;" N being in it taken as 86400.

Hence it appears that, taking δN as one vibration, the change in G corresponding to one vibration is

$$\frac{G}{43200} = G \times 0.000023148,$$
$$= sG \text{ (suppose)}.$$

The reduction of gravity for the height of the station may be taken as $g\,\dfrac{2h}{c}$, because the part of the reduction for height which would depend upon latitude is inappreciable.

Hence the correction for the reduction of gravity to the sea-level at Punnæ for 48 feet is

$$G \times 0.0000045875.$$

And the correction to be added to the vibration-number, by the formula for δN, will be

$$0.19818.$$

The observed vibration-number at Punnæ being 85982·75, we may say that at the sea-level at Punnæ,

$$N = 85982.95.$$

As an instance of the mode of making such a comparison as is proposed, let us take the case of Moré, the most northern station visited [*], being also the most elevated (15,408 feet). Moré is about 150 miles, by the map [†], distant from the sub-Himalayan plains to the south-west of it, and about the same distance N. by E. from Simla.

Then we shall have: Gravity at Moré — gravity at Punnæ

  = difference for difference of latitude,

   − difference for height,

   + difference for local attraction.

Now the difference for difference of latitude

$$= g(1 + \tfrac{1}{3}\epsilon - \tfrac{3}{2}m)(\tfrac{5}{2}m - \epsilon)(\tfrac{1}{2}\cos 2l' - \tfrac{1}{2}\cos 2l)\,[\ddagger].$$
$$= g\beta(\tfrac{1}{2}\cos 2l' - \tfrac{1}{2}\cos 2l), \text{ suppose,}$$

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[*] This was the last station visited by Captain Basevi, who there died, a martyr to his work. See 'Account &c.' p. x.

[†] Plate to "Account &c."

[‡] Pratt's 'Figure of the Earth,' 4th ed. art. 122, where his $\dfrac{E}{a^2}$ corresponds to our g.

where

$$\log \beta = \overline{3} \cdot 7159239.$$

But

Latitude of Punnæ = 8° 9′ 28″,

and of

Moré = 33° 15′ 39″.

The correction to gravity for difference of latitude then comes out

$$= g \times 0 \cdot 0014589.$$

Also correction for height

$$= -g \frac{2h}{c}.$$
$$= -g \times 0 \cdot 0014726.$$

Hence difference of gravity (δG) between Moré and Punnæ at sea-level ought to have been

$$g \times 0 \cdot 0000137.$$

Dividing this by $g \times 0 \cdot 0000023148$, which corresponds to one swing, it appears that there ought to have been 0·59 swing less at Moré, irrespective of local attraction, than at Punnæ.

Now the observed vibration-number at Moré was . 85984·62
And at Punnæ, when reduced to sea-level ...... 85982·95
Actual difference, δN ...... = + 1·67

So that there were in fact 1·67 swings more ; whereas the difference, irrespective of local attraction, ought to have been −0·5.

The result is that local attraction at Moré station needs to account for 2·26 swings in twenty-four hours.

Reducing this to attraction measure, we may accept as a fact that

Local attraction at Moré $= g \times 0 \cdot 000052386$,

where g is the attraction of the sphere.

Stations near the Meridian.	1. North latitude.	2. Height in feet.	3. Difference of vibration-numbers from 85982·95, being that at sea-level for Punnæ.	4. Calculated difference for height above sea and latitude, not regarding local attraction.	5. Number of swings to be accounted for by excess of local attraction over numbers at Punnæ at sea-level, i.e. (3)−(4).	6. Equivalent of last column in terms of sphere's attraction, $\times 10^{-7}$.	7. "Reduced height only" *minus* "reduced height and mass," being the attraction of the visible masses in vibrations.	8. Col. Herschel's estimate of the variation of gravity at sea-level, after allowing for attraction of the visible masses at sea-level.
Punnæ	8 9 ¼	48						
Kudankolam	8 10 21	168	− 0·39	− 0·47	+0·08	+ 19	0·26	+ 0·09
Mallapatti	9 28 45	288	− 0·35	+ 0·37	−0·72	− 166	0·44	− 1·12
Pachapaliam	10 59 40	971	− 0·67	− 0·36	−0·31	− 71	1·51	− 1·76
Bangalore, S.	13 0 41	3118	− 4·46	− 5·61	+1·15	+ 267	4·85	− 3·28
Bangalore, N.	13 4 56	3009	+ 3·57	+ 5·43	−1·86	− 429	4·68	− 2·79
Namthábád	15 5 52	1173	+ 4·76	+ 5·80	−1·04	− 241	1·83	− 2·89
Kodangal	17 5 57	1914	+ 8·06	+ 7·06	+1·00	+ 232	2·98	− 1·94
Damargida	18 3 17	1946	+ 8·09	+ 8·88	−0·79	− 183	3·03	− 3·91
Somtana	19 5 0	1714	+ 13·32	+ 12·39	+0·93	+ 216	2·63	− 1·66
Badgaon	20 44 23	1120	+ 19·31	+ 18·99	+0·32	+ 74	1·74	− 1·38
Ahmadpur	23 36 21	1693	+ 25·26	+ 24·47	+0·79	+ 183	2·57	− 1·73
Kalianpur	24 7 11	1763	+ 27·41	+ 25·66	+1·75	+ 406	2·74	− 0·96
Pahargarh	24 56 7	1641	+ 28·15	+ 28·57	−0·42	− 99	2·55	− 2·95
Usira	26 57 6	810	+ 38·36	+ 38·21	+0·15	+ 35	1·14	− 0·95
Datairi	28 44 5	717	+ 43·78	+ 44·47	−0·69	− 160	1·12	− 1·65
Kaliana	29 30 55	810	+ 44·30	+ 46·58	−2·28	− 528	1·25	− 3·49
Nojli	29 53 28	879	+ 44·67	+ 47·56	−2·89	− 670	1·36	− 4·23
Dehra	30 19 29	2242	+ 39·91	+ 43·41	−3·50	− 811	3·20	− 8·71
Mussoorie	30 27 41	6920	+ 28·64	+ 24·58	+4·06	+ 941	9·60	− 5·57
Moré	33 15 39	15408	+ 1·67	− 0·59	+2·26	+ 524	23·57	−21·44

The first six columns of the foregoing table involve no hypothesis beyond that the curve, which represents the Indian meridian, does not sensibly depart from that which best represents the Earth as a whole, which Colonel Clarke considers may be assumed*; and that the sea-level is not affected by local attraction. The fifth column is consequently the simple statement of the fact that local attraction at each station must be such as to account for so many swings of the pendulum *per diem* relative to the number at Punnæ reduced to sea-level, and must bear the ratio to the attraction of the sphere which is expressed in the sixth column. The seventh and eighth columns, deduced from the account of the pendulum-operations†, involve the further suppositions that the forms and positions of the attracting masses above the sea-level have been correctly estimated relative to the station, and that their density is half the mean density of the earth, the sea-level being supposed not to be disturbed by attraction. It will therefore appear that, although giving further information, they contain more elements of uncertainty than affect the numbers in the former columns. The numbers in the seventh column have been obtained from a tabular statement in the 'Account, &c.'† by subtracting the vibration-numbers in the column headed "Reduced height and mass" from those in the column headed "The reduced height only." It appears that this ought to give the effect at the station in vibration-numbers of the attraction of the masses elevated above the sea-level.

When we compare these numbers with those in column 5, it is seen at a glance how much smaller local attraction actually is than it might be expected to be. It is no doubt true that if allowance has to be made for a rise in the sea-level, it would make the heights of the masses above the mean level greater, and consequently diminish the numbers in column 4, and increase those in 5 and 6; but at the same time it would increase the masses referred to in column 7; so that the excess of the numbers in that column over those in column 5 would not be thereby explained. But, what is still more remarkable, is that local attraction is in some instances actually negative, where from the size and distribution of the masses it might be expected to be large and positive, as shown in column 7. This is notably the case, especially at Dehra, and to a less extent at Nojli; while at Moré, the most elevated station (15,408 feet), it is reduced from the amount which ought to be equivalent to cause 23·57 addi-

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* Account &c. p. xxxii.   † Ibid. p. [146].

tional swings of the pendulum *per diem* to what would produce so few as 2·26 only.

Let us now revert to the hypothesis of elevated masses of the Earth, produced by lateral compression, being accompanied by downward protuberances projecting into a denser substratum, and supported in a position of hydrostatic equilibrium; and let us inquire how the variation of gravity at the surface would be affected by such an arrangement of the masses. I call the downward protuberances the "roots" of the mountains. In my 'Physics of the Earth's Crust,' I have assumed the density of the mountain, of the crust, and of the root to be the same throughout; for since the density of more basic eruptive rocks is not much greater than that of acid rocks, there is no great margin left for supposing a gradation. It may be objected that the root could not remain protuberant, but would be melted off owing to the high temperature of the substratum. A probable answer to this is, that the root consisting of acid rocks is less fusible than the basic rocks of the substratum. Another is, that there has not been time; and this reason has more to be said in favour of it than appears at first sight, because we know that the upper mountain, although continuously degraded by atmospheric causes, is not at present levelled down. There may therefore equally well not have been time for the much larger root to have been melted off.

Professor Darwin has proved that the material of which the Earth is composed must be exceptionally rigid to support mountains, if (as I understand him) they are supported by rigidity alone. And since, with sufficient time given, every known substance yields more or less freely to stress, this is of itself an argument in favour of the hydrostatic theory. It is not necessary for the purpose that the substratum should be what would be called fluid; although I myself believe that it is so.

In discussing the effect upon the local variation of gravity arising from the supposed constitution of the crust, the first question which presents itself is, to what extent the sea-level will be affected. Pratt has an article (200) upon this; but the following proof is suggested.

Suppose generally that there is a mass, whose volume is M and density ρ, situated exterior to the Earth, and a mass, whose volume is R and density μ, within the Earth; and suppose that R is enveloped by the stratum whose density is σ.

Let $\dfrac{\rho M}{D}$ and $\dfrac{\mu R}{D'}$ be the potentials of the masses M and R at a point on the surface of the disturbed sea-level; $\dfrac{E}{r}$ the

potential of the Earth at the same point. Then, supposing the space occupied by R to be vacant, the potential of the Earth will become $\dfrac{E}{r} - \dfrac{\sigma R}{D'}$. Hence, when we take into account all the masses which contribute to form the potential, recollecting that their sum at every point at the surface of the ocean must be constant, we have, upon restoring R,

$$\text{constant} = \frac{\rho M}{D} + \frac{\mu R}{D'} + \left(\frac{E}{r} - \frac{\sigma R}{D'}\right),$$

$$= \frac{\rho M}{D} - \frac{(\sigma - \mu)R}{D'} + \frac{E}{r}.$$

At a distance from the masses where their attraction is inappreciable, r becomes c the mean radius.

Let $r = c + \delta c$, whence δc will be the rise of the sea-level. Then we have

$$\text{the constant} = \frac{E}{c};$$

$$\therefore \ \frac{E}{c} - \frac{E}{c + \delta c} = \frac{E}{c} - \frac{E}{c}\left(1 - \frac{\delta c}{c}\right) = \frac{\rho M}{D} - \frac{(\sigma - \mu)R}{D'},$$

or

$$\frac{E}{c^2}\delta c = \frac{\rho M}{D} - \frac{(\sigma - \mu)R}{D'}.$$

But $\dfrac{E}{c^2} = g$;

$$\therefore \ \delta c = \frac{1}{g}\left\{\frac{\rho M}{D} - \frac{(\sigma - \mu)R}{D'}\right\}.$$

If the included mass had been more dense than the enveloping stratum, we should have had

$$\delta c = \frac{1}{g}\left\{\frac{\rho M}{D} + \frac{(\mu - \sigma)R}{D'}\right\}.$$

We see, then, that the included mass R, if less dense, will have the effect of depressing the sea-level, and if more dense, of raising it. It may turn out, therefore, that our hypothesis of hydrostatic equilibrium will be found to accord with a very slight change in the sea-level.

There will be a slight change in the value of gravity at the depth occupied by the root : let its mean value be supposed to occur at the middle part, and be g'. Then, remembering that the attraction of the spherical shell exterior to the point

in question is *nil* at that point, we have approximately,

$$g' = \frac{E - 4\pi \left\{ \left(c - \frac{k}{2} \right)^2 k\rho + \left(c - k - \frac{t}{2} \right)^2 t\sigma \right\}}{\left(c - k - \frac{t}{2} \right)^2}.$$

Taking the mean density to be $5\frac{1}{2}$, we have

$$\frac{4}{3} \pi \frac{11c}{2} = \frac{E}{c^2} = g ;$$

whence, neglecting terms in $\frac{1}{c^2}$,

$$g' = \left\{ 1 - \frac{1}{c} \left(\frac{6}{11} (k\rho + t\sigma) - (2k + t) \right) \right\}$$
$$= g \{ 1 - \alpha \},$$

where α is a small fraction.

We will first consider the conditions of hydrostatic equilibrium in the case of a spherical cap as already defined, such as has been made the basis of the calculations of Archdeacon Pratt and of Colonel Herschel, making the additional hypothesis respecting the support of the mountain which has been proposed. The elevated mass will therefore be considered as of the form of a spherical cap, having its root also of that form. We will for the present consider the root as contained within the same verticals as the mountain; that is, they are both frustra of the same cone, whose vertex is the centre of the earth. The conditions of equilibrium obtain for the cone. This supposition ignores the rigidity of the crust, which may appear a violent assumption to those who are not geologists. It is now, however, generally admitted that the materials of the earth's crust have yielded very freely to the stresses which have affected them ; and that the phenomena of schistosity, cleavage, contortion, and of other kinds of metamorphism, are evidences of this fact. In short, the materials are of the nature of more rigid portions imbedded in less rigid media, the shapes and magnitude of the more rigid, and the ductility of the less rigid varying both absolutely and relatively in every possible degree. Thus all rocks appear to be more less of a viscous nature ; that is to say, when subjected to stress for a sufficiently long period, they become sensibly strained out of their original forms and relative positions. And under these circumstances chemical reactions are also set up. It will appear in the sequel that rigidity does play some part in the equilibrium of the crust; but it may be for the present supposed that the conditions of equilibrium are satisfied throughout

every elementary pyramid, having its vertex at the Earth's centre.

The root being contained within the same verticals, taking w as the chord of the semiarc at the middle of it, we have

$$\frac{w}{u} = \frac{c - \left(k + \frac{t}{2}\right)}{c};$$

and, the crust being independently in equilibrium, we must have for the support of the mountain by the root,

$$\frac{w^2}{u^2} = 1 - \frac{2\left(k + \frac{t}{2}\right)}{c}, \text{ approximately,}$$

And since $g' = g(1 - \alpha)$,

$$\rho h = (\sigma - \mu)(1 - \alpha)\left(1 - 2\frac{k + \frac{t}{2}}{c}\right)t,$$

$$= (\sigma - \mu)\left(1 - \alpha - 2\frac{k + \frac{t}{2}}{c}\right)t,$$

where α has the value already found ; substituting which

$$\rho h = (\sigma - \mu)\left(1 - \frac{1}{c}\frac{6}{11}(k\rho + t\sigma) + 2\frac{k + \frac{t}{2}}{c} - 2\frac{k + \frac{t}{2}}{c}\right)t,$$

$$= (\sigma - \mu)\left(1 - \frac{1}{c}\frac{6}{11}(k\rho + t\sigma)\right)t,$$

$$= (\sigma - \mu)(1 - \tau)t ; \text{ suppose.}$$

This is a quadratic to determine t.

As a first approximation, $\rho h = (\sigma - \mu)t$ (of course).

To find the rise of the sea-level under the supposed conditions. Pratt says that, " for problems of this kind, the Himalayas may be considered as a vast tableland about three miles high."[*] The spherical cap already described as forming the basis of the calculations in that case will become a cylinder.

First, then, suppose a cylinder of radius u, and small height, standing upon the surface of the mean sphere.

* Page 213.

Let C be on the true sea-level, Q on the disturbed surface, P the station, C Q = z, Q P = h the observed height of the station; because the levels of the theodolite will always be parallel to what the surface of the water would be, were there a canal cut from the coast to beneath the station.
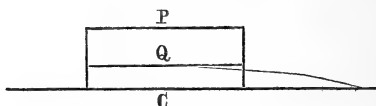
Then we require the potentials of the two portions, C Q and P Q, of the cylinder at the point Q. That of C Q, of height z, will be found to be

$$2\pi\rho \left\{ \frac{z}{2}\sqrt{z^2 + u^2} - \frac{z^2}{2} + \frac{u^2}{2} \log \frac{z + \sqrt{z^2 + u^2}}{u} \right\}.$$

If we expand, and neglect terms in $\dfrac{z^2}{u^2}$, this becomes

$$2\pi\rho \left\{ zu - \frac{z^2}{2} + \frac{1}{6} z^2 \frac{z}{u} \right\}.$$

Pratt neglects the last term, in which we shall follow him.

The potential of P Q at Q will be of the same form, only having $h - z$ in the place of z: and that of the root may also be got from the above expression, *mutatis mutandis.* Hence, treating for the present purpose the plateau and the root as cylinders of radii u and w respectively, we have the whole potential at Q made up of that of the sphere, + that of the cylinder of thickness $h - z$, radius u, and density ρ, at a point at the end of its axis, + that of the cylinder of thickness z, radius u, and density ρ, at a point at the end of its axis, — that of the cylinder of thickness t, radius w, and density $\sigma - \mu$, at a point distant $z + k$ from its end. Treating the last as the difference of two potentials, we have by what has been already proved,

$$g \times \text{ rise of the sea-level} = gz = 2\pi\rho \left((h-z)u - \frac{(h-z)^2}{2} \right)$$

$$+ 2\pi\rho \left(zu - \frac{z^2}{2} \right)$$

$$+ 2\pi(\sigma - \mu) \left\{ (z+k+t)w - \frac{(z+k+t)^2}{2} - \left((z+k)w - \frac{(z+k)^2}{2} \right) \right\}$$

$$= 2\pi\rho \left(hu - \frac{1}{2}(h^2 - 2hz) - z^2 \right) + 2\pi(\sigma - \mu) \left\{ tw - (z+k)t - \frac{t^2}{2} \right\};$$

whence, remembering that $\dfrac{g}{2\pi} = \dfrac{11}{3}\, c$,

$$z\left(\frac{11}{3}c - h - \frac{\sigma - \mu}{\rho}\, t\right) + z^2 = hu - \frac{h^2}{2} - (\sigma - \mu)\left(tw - kt - \frac{t^2}{2}\right),$$

which shows that

$$z < \frac{hu - \dfrac{h^2}{2} - \dfrac{\sigma - \mu}{\rho}\left(tw - kt - \dfrac{t^2}{2}\right)}{\dfrac{11}{3}\, c - h - \dfrac{\sigma - \mu}{\rho}\, t}.$$

When the masses are regarded as cylindrical, $u = w$, and $g\rho h = g'(\sigma - \mu)t$; g' being less than g by a quantity of the order of $\dfrac{1}{c}$, which, on account of c in the denominator of the fraction, may be neglected; and, finally,

$$z < \frac{\dfrac{\sigma - \mu}{\rho}\left(kt + \dfrac{t^2}{2}\right) - \dfrac{h^2}{2}}{\dfrac{11}{3}\, c - h - \dfrac{\sigma - \mu}{\rho}\, t}.$$

If we give to h the extreme value of 5 miles, and make $k = 25$ miles, $t = 50$ miles, and putting $\mu = \rho = 2.68$, and $\sigma = 2.96$, $c = 3953$ miles, we get

<div align="center">Rise of sea-level less than 90 feet.</div>

But if we take the height of the plateau at three miles, the rise of the sea-level will be only 17 feet. These are very much less than former estimates [*]. If what precedes is correct, we are justified in accepting the observed heights of the stations as their true heights [†].

It will be noticed that the condition of equilibrium has caused u, the radius of the plateau, to disappear from the expression for the rise of the sea-level. It does not, however, follow that the same rise would be occasioned by a conical or dome-shaped mountain as by an elevated plateau, because the result has been obtained on the supposition that the radius is

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[*] I am unable to follow Archdeacon Pratt, where he seems to state that an attenuation will raise the sea-level at a station above it, and makes the rise at Moré nearly 1000 feet (p. 214, 4th ed.).

[†] It follows from the above that, in the case of extensive floating fields of ice, their effect to raise the sea-level around them would be inconsiderable. An ice-cap resting upon the sea-bottom might be regarded as a mountain of ice, and its effect estimated accordingly.

large in comparison with the height. Any special case—such, for instance, as that of the island of Hawaii—would need to be considered on its own merits, regard being had to the totally different character of its constitution, owing to its not having been accumulated through lateral compression.

We are now in a position to find an expression for the vertical local attraction arising from a cap-sector and its root. "A cap-sector and a zone-sector are corresponding portions of a cap and of a zone, intercepted between two planes passing through the axis, and inclined to each other at the sector-angle," * α.

The expression for the attraction of a cap-sector of thickness h, chord-radius u, and density ρ, is

$$\rho\alpha\left(u + h - \sqrt{u^2 + h^2} + \frac{uh}{2c} - \frac{h^2}{c}\right)\dagger,$$

neglecting terms in $\frac{1}{c^2}$ and $\frac{1}{uc}$.

To express this local attraction in terms of g, the attraction of the sphere, we shall have, putting the mean density at $5\frac{1}{2}$,

$$\text{Local attraction} : g \; :: \; \rho \times \&c. \; : \; \frac{E}{c^2},$$

$$:: \; \rho \times \; \&c. \; : \; \frac{4}{3}\pi \times \frac{11c}{2},$$

$$:: \; \rho \times \; \&c. \; : \; \frac{2\pi}{\gamma} \; (\text{suppose}).$$

And if c is expressed in miles, we shall find that

$$\log \gamma = \overline{5}\cdot 8384723.$$

Whence, in terms of the attraction of the sphere,

$$\text{Local attraction} = \frac{g\gamma}{2\pi} \rho \times \&c.$$

The negative attraction of the root will be that of a cap-sector of thickness t and of radius w, at a distance $h + k$, = that of a cap-sector of radius u and thickness $h + k + t$, − that of a cap-sector of radius u and thickness $h + k$. The radius w therefore does not appear. The chord-radius u may be taken the same for the under as for the upper side of the plateau.

Hence the negative attraction of the root

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* "Account" &c. p. [151].   † Ibid. p. [157].

$$= (\sigma - \mu)\alpha \left\{ u + h + k + t - \sqrt{u^2 + (h+k+t)^2} + \frac{u(h+k+t)}{2c} \right.$$

$$\left. - \frac{(h+k+t)^2}{c} - \left(u + h + k - \sqrt{u^2 + (h+k)^2} + \frac{u(h+k)}{2c} - \frac{(h+k)^2}{c} \right) \right\}$$

$$= (\sigma - \mu)\alpha \left\{ t - \sqrt{u^2 + (h+k+t)^2} + \sqrt{u^2 + (h+k)^2} + \frac{ut}{2c} \right.$$

$$\left. - \frac{2(h+k)t + t^2}{c} \right\}.$$

So that the whole vertical attraction of the cap-sector at the extremity of its axis is

$$\rho\alpha \left\{ u + h - \sqrt{u^2 + h^2} + \frac{uh}{2c} - \frac{h^2}{c} \right\}$$

$$- (\sigma - \mu)\alpha \left\{ t - \sqrt{u^2 + (h+k+t)^2} + \sqrt{u^2 + (h+k)^2} + \frac{ut}{2c} \right.$$

$$\left. - \frac{2(h+k)t + t^2}{c} \right\}.$$

But the condition of hydrostatic equilibrium gives

$$\rho h = (\sigma - \mu)(1 - \tau)t \; ;$$

and it will appear that fractional quantities in $\frac{1}{c}$ may be neglected.

$$\therefore \; \rho h = (\sigma - \mu)t.$$

Hence, considering an elementary sector of axial angle $\delta\alpha$, its attraction will be

$$\rho\delta\alpha(u - \sqrt{u^2 + h^2}) + (\sigma - \mu)\delta\alpha(\sqrt{u^2 + (h+k+t)^2}$$
$$- \sqrt{u^2 + (h+k)^2}),$$

the terms in h, t, uh, and ut cancelling, by the condition of equilibrium. Thus the first part of Young's correction disappears.

The above expression affords a simple criterion of the amount of the vertical attractive force at the extremity of the axis, arising from the disturbance of gravity by the matter constituting a cap-sector, on the supposition of hydrostatic equilibrium. It is proportional to the difference between the extreme distances of the upper and lower edges of the root, multiplied by the relative density thereof, diminished by the difference of the distances of the upper and lower extreme edges of the visible mass, multiplied by its density. But when the distances are great, the thickness of the visible mass being small compared with that of the root, the attraction may be regarded as practically proportional to the difference of the distances of the upper and lower edges of the root. It follows that the attraction is less the longer the sector, and becomes insensible when that is very long.

Suppose an elevated plateau of the form of an infinitely long parallelepiped, and that the vertical attraction at a point on its surface, at a distance a from one edge and b from the other, is required. Let α be the angular distance of a sectorial radius from a. Then, dividing the plateau into two portions by a longitudinal plane through the station, and putting u successively $= a \sec \alpha$ and $= b \sec \alpha$, and calling the above expression for the attraction $f(a, \alpha)$ and $f(b, \alpha)$, and introducing the unit-factor $\dfrac{g\gamma}{2\pi}$, we shall have

$$\text{Vertical attraction} = \frac{g\gamma}{2\pi} \, 2 \int_0^{\frac{\pi}{2}} (f(a, \alpha) + f(b, \alpha)) d\alpha.$$

A rather tedious integration gives

$$\int \left(\sqrt{a^2 \sec^2 \alpha + (h+k+t)^2} - \sqrt{a^2 \sec^2 \alpha + (h+k)^2} \right) d\alpha$$

$$= \frac{a}{2} \log_e \frac{\tan^2 \alpha + \frac{1}{2}\left(1 + \left(\frac{h+k+t}{a}\right)^2\right) + \sqrt{1 + \left(\frac{h+k+t}{a}\right)^2 \tan^2 \alpha + \tan^4 \alpha}}{\tan^2 \alpha + \frac{1}{2}\left(1 + \left(\frac{h+k}{a}\right)^2\right) + \sqrt{1 + \left(\frac{h+k}{a}\right)^2 \tan^2 \alpha + \tan^4 \alpha}}$$

$$+ (h+k+t) \sin^{-1} \frac{\sin \alpha}{\sqrt{1 + \left(\frac{a}{h+k+t}\right)^2}} - (h+k) \sin^{-1} \frac{\sin \alpha}{\sqrt{1 + \left(\frac{a}{h+k}\right)^2}}.$$

And this, when taken from $\alpha = 0$ to $\alpha = \dfrac{\pi}{2}$, gives

$$- \frac{a}{2} \log_e \frac{1 + \left(\frac{h+k+t}{a}\right)^2}{1 + \left(\frac{h+k}{a}\right)^2} + (h+k+t) \sin^{-1} \frac{1}{\sqrt{1 + \left(\frac{a}{h+k+t}\right)^2}}$$

$$- (h+k) \sin^{-1} \frac{1}{\sqrt{1 + \left(\frac{a}{h+k}\right)^2}}.$$

From which $\displaystyle\int_0^{\frac{\pi}{2}} (a \sec \alpha - \sqrt{a^2 \sec^2 \alpha + h^2}) d\alpha$ may be obtained by putting $h+k=0$ and $t=h$, and changing the signs.

I have calculated the vertical attraction, using these expressions; putting $h = 2\cdot915$ miles (the height of Moré), $k = 25$, $t = 27\cdot925$, $\rho = \mu = 2\cdot68$, $\sigma = 2\cdot96$, $a = 80$, $b = 400$. The result is that the attraction would produce $4\cdot155$ swings of the pendulum *per diem*.

To illustrate how much increasing the distance of the station from the edge of the plateau diminishes the attraction, it may be mentioned that the former of the above integrals gives, for $a = 80$,

> The logarithmic term $= -11 \cdot 274$,
> The two circular terms $= +24 \cdot 651$;

whereas for $a = b = 400$ they are

> The logarithmic term $= -2 \cdot 888$,
> The two circular terms $= +5 \cdot 807$.

In like manner the latter integral gives, for $a = 80$,

> The logarithmic term $= +0 \cdot 0519$,
> The circular term $= -0 \cdot 1061$;

whereas for $a = b = 400$ these are respectively

> $+0 \cdot 00798$ and $-0 \cdot 0185$.

As another instance of the effect of form and extension, it will be found that a circular plateau of the above height and of 80 miles radius (and therefore of infinitely smaller area than the parallelepiped) will, with its root, produce an attraction corresponding to about 11 swings *per diem.*

Archdeacon Pratt, in " a postscript on Himalayan attraction," in the Phil. Trans. 1859, p. 774, gives the rough diagram of the plateau here reproduced, referring to a work on the physical geography of the Himalayas by Major R. Strachey, adding :—" On a careful consideration of all the data, Captain H. Strachey estimates the mean elevation of the tableland between the Himalayan and Turkish watersheds, and to the west of the ridge between the sources of the Indus and Brahmaputra, to be 15,000 feet (p. 56) "*. This is the height of the station Moré.

As far as I can judge by comparing the map in the 'Account' &c., the only one in which the locality is marked, with the relief-map by Major G. Strahan, and with Pratt's diagram, it appears that Moré is distant about 80 miles from the nearly straight south-western escarpment of the range facing the plains, and 400 miles from the escarpment facing the north-west. If we imagine a series of cap-sectors drawn about the station, it will appear that the hypothesis of the parallelepiped makes the sectors on the north side too short near the cross section, and on the south side too long, while parallel to the range they are necessarily too long. This would make the attraction at the station too small. On the other hand, the actual plateau rises from the plains which are

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* *Loc. cit.* p. 776.

about 800 feet high, whereas the parallelepiped has been taken as rising from the sea-level. This would make its attraction too great. The difference could be calculated, if worth while.
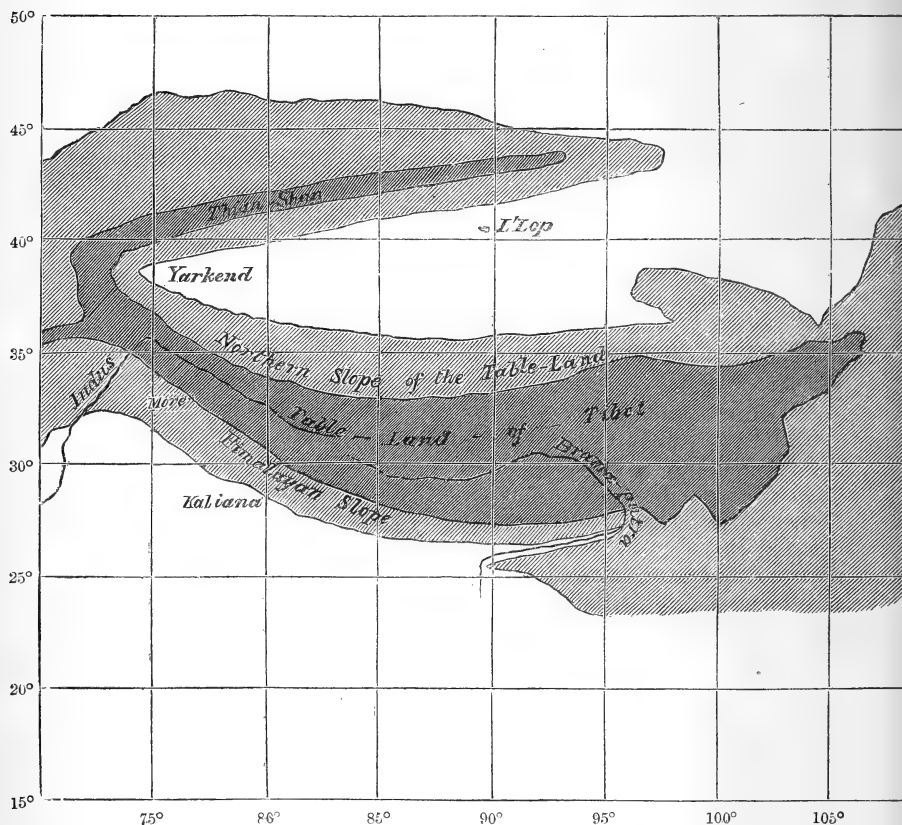
Further, the slope of the range ought to have been taken into account. Thus a close agreement is not to be expected. However, referring to the fifth column of the Table, it appears that local attraction at Moré relative to Punnæ ought to produce 2·26 swings *per diem* ; whereas the attraction of our parallelepiped at a station situate as supposed would produce 4·15, or nearly two swings more. The visible masses were estimated as being capable of producing 23·57 swings. Hence the hypothesis of the parallelepiped, hydrostatically supported, accounts for 19·42 out of the 21·31 swings in defect which

have to be accounted for. So that the theory of hydrostatic equilibrium in this instance may be considered a not unsatisfactory explanation of the phenomena.

To understand what has been done, we observe that the calculated effect of the local attraction of the parallelepiped is absolute, not relative, supposing that there would be no local attraction at the mean sea-level of a continental land-surface. It ought therefore to be compared with an absolute quantity. The 23·57 swings, given in column 7 of our Table, being the difference between the effect due to height and mass at Moré and that due to height alone, is also absolute. If, therefore, we take the parallelepiped to roughly represent the plateau, 23·57 − 4·15 = 19·42 is the absolute difference between the effect of the plateau considered as supported by an excessively rigid crust and by hydrostatic equilibrium. Thus far observed vibration-numbers are not involved.

When we consider these, we see that there were 1·67 more swings at Moré than at Punnæ. But without knowing what the effect of local attraction is at Punnæ, we can have no exact knowledge of that which exists at Moré. If the number of swings of a given pendulum at the equator at the surface of the sea (but not subjected to the local attraction of an insular mass) was known, knowing the number at Punnæ, we could deduce the absolute attraction there, and consequently at Moré also. Here, however, we are at fault.

Colonel Herschel has treated this question very fully in the second Appendix to the 'Account, &c.,' explaining that he has adopted a novel method ; for, whereas local variations of gravity had previously been treated by the method of least squares, as if they were errors of observation, he has regarded them as having a real existence ; and has obtained his equations for finding the vibration-number of a given pendulum at the equator from the numbers observed at known stations, on the two suppositions (1) that the mean of all observed local variations should be zero, and (2) that the mean of those in one narrow zone of north latitude should be equal to the mean of those in the corresponding zone of south latitude.

But, seeing that the local variations are calculated subject to the attraction of the masses, and that the masses give very different attractions according as they are treated as if supported by excessive rigidity of the crust, or by hydrostatic equilibrium, it appears that the resulting equatorial mean vibration-number obtained on the one supposition is not likely to agree with that obtained upon the other.

Colonel Herschel (Appendices, p. 45) makes local attraction at Punnæ = − 4·2 swings. But if, by way of illustration, the

hypothesis of hydrostatic equilibrium should turn out to so alter the equatorial vibration-number as to make it instead $= +1\cdot89$, then the absolute local attraction at Moré would be $1\cdot89 + 2\cdot26 = 4\cdot15$, the same as that of our parallelepiped hydrostatically supported.   This is merely by way of illustration, because the parallelepiped only roughly represents the plateau; but it shows that the hypothesis may probably be competent to explain the phenomena.

If u is larger than $h + k + t$, the expression, whose integral is given at p. 18, may be put under the approximate form

$$h\int\frac{2k+h+t}{2a\sec\alpha}d\alpha.$$

But since a is in the present case but little the larger, I have thought it best to calculate the attraction from the fuller formula.   It seems, however, that the approximate formula would have given a result sufficiently exact ; for if we put a successively equal to 80 and 400 miles, using the above approximate formula, we get the result $4\cdot51$ swings, whereas the fuller formula gives $4\cdot15$.   These two numbers are sufficiently near for our purpose.

Relying upon this, we can estimate the local vertical attraction at Kaliana.

The attraction of a cap-sector upon a point beneath it, estimated downwards, is

$$-\rho\delta\alpha\left(u+h-\sqrt{u^2+h^2}-\frac{uh}{2c}\right);$$

and of its root in the same direction,

$$-(\sigma-\rho)\delta\alpha\left(t-\sqrt{u^2+(h+k+t)^2}-\sqrt{u^2+(h+k)^2}+\frac{ut}{2c}\right).$$

The condition of equilibrium gives $(\sigma-\rho)t=\rho h$.   Hence the curvature terms cancel ; and, expanding in terms of $\dfrac{h+k+t}{u}$, $\dfrac{h+k}{u}$, and $\dfrac{h}{u}$, we get

$$\text{Attraction} = -\rho h\delta\alpha\left(2-\frac{3h+2k+t}{2u}\right) \text{ approximately.}$$

Now it appears from Major George Strahan's Relief-map of India that the Himalayan plateau presents a nearly straight escarpment towards Kaliana, this straight face subtending an angle of about 120° at that station, so that radii drawn beyond · this will not enter the plateau on the western, and at a long distance only on the eastern side.

Suppose, then, a series of cap-sectors, each of axial angle δa, but of different radii, respectively long enough to reach the further edge of the plateau; and let a triangular block be cut off from these by a vertical plane through the crest of the escarpment. The vertical attraction of the mass beyond the vertical plane at the station will be that of the aggregate of the sectors, *minus* that of the triangular block. There will remain the attraction of the slope of the plateau to be added.
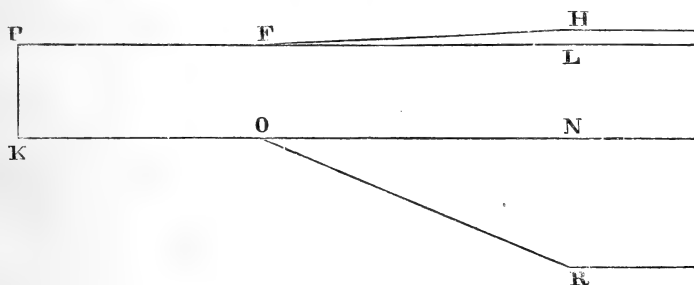
Scale $\frac{1}{10}$ inch = 5 miles.

P the station Kaliana.

H the commencement of the plateau.

H L$=h=2\cdot815$ miles.

F the foot of the slope.

P L$=a=140$ miles.

P F$=b=60$ miles.

N R$=$depth of root of H L$=t=9\cdot57 \times 2\cdot815$ miles.

P K$=k=26\cdot621$ miles.

It will be observed that this value of k includes the height of the station, $0\cdot153$ mile, and of its root, $1\cdot468$ mile.

The diagram shows a section through Kaliana at P, taken along the medial plane of the cap-sectors, and is intended to be on a scale of $\frac{1}{10}$ of an inch to five miles. The plateau commences at H. NR is the depth of its root. PK is thickness of the crust, including the plain on which the station stands and its root. FH is the slope of the plateau. OR the slope of the corresponding root.

In calculating the attraction of the plateau and its root, we may reckon the height of the plateau from the level of Kaliana, leaving out of consideration the attraction of the plain on which the latter stands, which may be regarded as an infinite plain extending under the plateau, and having its attraction balanced by that of its own root.

The attraction of the plateau will then be

$$-\Sigma \rho h \delta \alpha \left(2 - \frac{3h + 2k + t}{2u} \right) - \text{attraction of the triangular block.}$$

And a being the distance of the edge of the plateau from Kaliana,

Attraction of the triangular block

$$= -2\rho h \int_0^{\frac{\pi}{3}} \left(2 - \frac{3h + 2k + t}{2a \sec \alpha} \right) d\alpha,$$

$$= -2\rho h \left(2\frac{\pi}{3} - \frac{\sqrt{3}}{2} \frac{3h + 2k + t}{2a} \right). \quad . \quad . \quad . \quad (A)$$

But

$$\Sigma \delta \alpha = 2\frac{\pi}{3} ;$$

∴ the attraction of the plateau

$$= -2\rho h \frac{\sqrt{3}}{2} \frac{3h + 2k + t}{2a} + \Sigma \rho h \delta \alpha \frac{3h + 2k + t}{2u}.$$

To this has to be added the attraction of the slope of the plateau, which we must now calculate. An inspection of the diagram shows that the depth to the bottom of the root $(h + k + t)$ is always less than u, the distance from the station, which justifies the use of the approximate formula.

Writing η for h and $n\eta$ for t, we have from (A),

Attraction of the triangular block of height η

$$= f(\eta) = -2\rho \left(\frac{2\pi}{3} \eta - \frac{\sqrt{3}}{2} \frac{(3 + n)\eta^2 + 2k\eta}{2a} \right).$$

And it is evident that the attraction of the elementary layer of height $\delta \eta$ with its root will be

$$\frac{df(\eta)}{d\eta} \delta \eta = -2\rho \left(\frac{2\pi}{3} - \frac{\sqrt{3}}{2} \frac{(3 + n)\eta + k}{a} \right) \delta \eta.$$

The end of the slice $\delta \eta$, on account of the slope, will be distant from the station,

$$b + \frac{\eta}{h}(a - b);$$

so that, for its attraction at P, we must substitute this quantity for a, and subtract, and the attraction of the slope and its root will be

$$\int_0^h -2\rho \left\{ \frac{\sqrt{3}}{2} \frac{(3 + n)\eta + k}{b + \frac{a - b}{h}\eta} - \frac{\sqrt{3}}{2} \frac{(3 + n)\eta + k}{a} \right\} d\eta,$$

$$= -2\rho \left(\frac{\sqrt{3}}{2} \frac{(3+n)h}{a-b} h - \left(\frac{bh}{a-b} - \frac{k}{3+n} \right) \log_e \frac{a}{b} \right.$$
$$\left. - \frac{\sqrt{3}}{2} \frac{(3+n)h^2 + 2kh}{2a} \right).$$

Adding this to the attraction of the plateau, since the second term of the one cancels the first of the other, we have the attraction at the station

$$= -\rho \sqrt{3} \frac{h}{a-b} \left\{ (3+n)h - \left(\frac{(3+n)hb}{a-b} - k \right) \log_e \frac{a}{b} \right\}$$
$$+ \Sigma \rho h \delta \alpha \frac{(3+n)h + k}{2u}.$$

If we substitute the values indicated above, and introduce the factor $\frac{\gamma}{2\pi s}$*, to express the effect in swings, the former of the above terms gives $-2\cdot73$ swings in defect, due to the attraction of the plateau and other masses. The second term is not easily estimated, on account of the irregular outline of the further boundary of the plateau ; but it will certainly be quite small, but being positive will make the negative effect less. Now there were observed relative to Punnæ $-2\cdot28$ swings in defect due to local causes at Kaliana.

The estimate $-2\cdot73$, having been obtained on the supposition that the slope of the plateau is a solid inclined plane, will be lessened by the circumstance that it is much intersected by valleys ; and this will be equivalent to assigning a lower mean value to the density ρ. Thus, on the whole, we may conclude that the calculated effect of local attraction at Kaliana brings the negative variation of gravity very close to the observed variation relative to Punnæ.

The above number has (as was also done in the case of Moré) been estimated on the suppositions that the ellipticity is correctly assumed, and that there would be no local attraction at the mean surface of the crust k considered as a land surface. Could we suppose Punnæ to be in such a situation, the above comparison would be exact. But since it is on the sea-coast, there is probably some local attraction there.

In all but six instances the local attractions, given in the 'Account &c.' as due to the visible masses, have been estimated as if they were caused by an infinite plain of the height of the station. The exceptions are Somtana, Ahmadpur, Usira, Dehra, Mussoorie, and Moré, at which places allowance has been made for inequality of surface. It might be thought that we need not have estimated the attractions afresh, as has

* See pages 6 and 16

been done roughly for Moré and Kaliana, because they have been already calculated and published.   But it will be observed that the attraction of the " roots " of the masses cannot be discovered from that of the masses themselves, except in the case of plains.

At the two former of the " very irregularly surrounded stations," Dehra, Mussoorie, and Moré, we shall make no attempt to calculate the attractions, and conclude with some remarks upon certain stations in Peninsular India.

There are no true mountain-ranges in Peninsular India, the so-called " mountains " being only the escarpments of plateaus which have escaped denudation.   " Peninsular India is, in fact, a tableland, worn away by subaerial denudation, and perhaps to a minor extent on its margins by the sea." *   The Deccan traps are of Lower-Eocene age, covered in places by nummulitic rocks†.   Their total thickness may be 6000 feet ‡. The horizontality of the flows in these plateaus is remarkable. In considering the bearing of the gravitational phenomena at stations in this part of India, we ought to take its structure into account.   No root has been formed by compression during the formation of its hills.   As the country became gradually weighted by flow upon flow of the basalts, the crust must have sunk gradually into the magma.   The Geological Survey does not appear to have yet mastered the details ; but possibly it will be found that the country is faulted, and consequently deep roots will answer rather to low elevations than to high ones, and the equilibrium will be of the tract as a whole, instead of being established within every vertical boundary.

In any case where the attraction of the mass above the sea-level may be justly taken as due to an infinite plain, it appears by our formula that the negative attraction of the root would, on the hypothesis of hydrostatic equilibrium being established within every vertical boundary, exactly balance it ; and the resulting local attraction at the station ought to be *nil*.   Now there are seven stations of the great arc between latitudes 16° and 24° N. which are upon the basalt, viz. from Pahargarh to Kodangal inclusive ; and the local attractions, relative to the attraction at sea-level at Punnæ, in swings of the pendulum, range for these stations, as shown in column 5, from −0·79 to +1·75, being as below :— .

* 'Manual of the Geology of India,' by Medlicott and Blanford (Calcutta, 1879), p. v.   The contrast between the peninsular and Himalayan regions is strikingly shown by a large model now on view in the Indian Annexe of the Indo-Colonial Exhibition at South Kensington.
† Ibid. p. 381.   ‡ Ibid. p. 308.

Heights.	Station.	Local attraction relative to Punnæ.
ft.		
1914	Kodangal	+1·00
1946	Damargida	−0·79
1714	Somtana	+0·93
1120	Badgaon	+0·32
1693	Ahmadpur	+0·79
1763	Kalianpur.........	+1·75
1641	Pahargarh.........	−0·42
Mean 1685	....	+0·42

These numbers will be uniformly increased or diminished by any local attraction there may be at Punnæ, which is not situated on an infinite plain, but where proximity to the coast places it under conditions different from those at the stations which we are referring to it.

To appreciate what these differences from zero attraction at the stations imply, we observe that one vibration *per diem* due to local attraction corresponds to about 645 feet of elevation of a plain ; and therefore, since the root (supposed of density ρ) displaces a layer of density σ, one vibration in defect caused by it will correspond to $\frac{\rho}{\sigma-\rho} \times 645$, or 6172 feet, *i. e.* to 1·1707 mile of root.

We see, then, that the root at Damargida, where the attraction is −0·79, would be about 0·92 mile too deep for local equilibrium ; and at Kalianpur, where it is +1·75, it would be 2·046 miles too shallow—these estimates being of course subject to the uncertainty belonging to local attraction at Punnæ.

If we assume the crust at Punnæ to be 25 miles thick (which I have shown in my ' Physics &c.' to be probable for places on the sea-coast), then, the depth for zero attraction of the root at Damargida being 3·527 miles, the actual depth there would be 5·171 and the whole thickness of the crust 30 miles. At Kalianpur, the depth for zero attraction being 3·195 miles, the actual depth would be 1·149 and the whole thickness 26 miles. These two instances are the greatest variations that occur throughout 8 degrees of latitude, and there are but two others of similar amount among the sixteen stations between Kaliana and Punnæ.

Although these varying local attractions at the several stations make it clear that this region is not in hydrostatic equilibrium everywhere locally, nevertheless, the relative attractions being at some stations positive and at others negative, it is quite possible that it may be as a whole sup-

ported in that manner, because we do not know how large the areas may be which would show positive or negative relative attraction under column 5. At any rate, the mean relative to Punnæ being only 0·42 swing, the result does not seem to discredit the hypothesis that there is a distribution of matter not far differing from what would accord with equilibrium for the region as a whole. The attractions at these peninsular stations give no information about the *mean* thickness of the crust, assumed at 25 miles, because, in the case of an infinite plain, the terms involving it (k) are neglected.

On the whole, it is apparent that the bottom of the crust is here irregular, and does not locally correspond for equilibrium with the surface-contour. In a basaltic region, as already remarked, this would seem natural ; for there will have been no compressing action tending to produce downward bulges corresponding to the elevated tracts, as would be the case in a mountain-chain ; so that any downward projection into the magma (*i. e.* root) will be due simply to the local depression of the crust, owing to its having become overweighted at the top, while rigidity of the crust, never crushed as in a mountain-chain, would extend the depression laterally and diminish it vertically. It is possible that the considerable local thickenings and thinnings of the crust, which appear to occur at a few places in this region, may be due to faults of large throw, such as are not unknown in countries where there has been much out-pouring of basalts.

The object of this paper has been (1) to summarize some of the results obtained during the Indian perdulum operations, as far as they bear upon the problem of the constitution of the Earth's crust ; and (2) to inquire whether, in a few of the instances where it seemed practicable to approach the subject by way of average, the theory of hydrostatic equilibrium gives a fair explanation of the phenomena. The cases that have been thus examined show that the theory of hydrostatic equilibrium makes, in swings of the pendulum,

(1) Local attraction at Moré . . . (about)   4·15
　　Whereas relative to Punnæ it is . . . .   2·26
　　　　　　　　Difference . . . . .   1·89
(2) It makes local attraction at Kaliana (about) −2·73
　　Whereas relative to Punnæ it is . . . .   −2·28
　　　　　　　　Difference . . . . .   −0·45
(3) It makes mean of attraction at 7 stations
　　　　on the basalt . . . .   0·00
　　Whereas relative to Punnæ it is . . . .   0·42
　　　　　　　　Difference . . . . .   −0·42

It is submitted that, considering the nature of the subject, these results are fairly confirmatory of the theory, and also of the assumed values of the densities, and to a less extent of that of the mean thickness of the crust at the sea-coast.

Geological changes of level are closely connected with the theory of hydrostatic equilibrium, as I have pointed out in my 'Physics of the Earth's Crust,' chap. xvii. When the effects of contour have been allowed for, we ought to find a general negative variation of gravity in such regions as are now disposed to rise, because that would indicate that the root is too deep for equilibrium. And for the opposite reason, a positive variable ought to be met with where depression is in progress. Whether this relation holds has, I suppose, not been noticed. Again, if the hypothesis is true, beneath plains the increment of underground temperature ought to be more rapid where the variation of gravity is positive, and more slow where it is negative.

Postscript.—Since the foregoing was sent to press, I have read M. Faye's article, " Sur la Constitution de la Croute Terrestre," in *Comptes Rendus* for March 22, 1886. He there discusses pendulum-observations at island and continental stations, the latter with especial reference to the Indian observations. Respecting the excess of gravity found at island stations, he comes to a conclusion similar to that arrived at by Pratt in reference to Minicoy (art. 74). With regard to continents, his views likewise agree generally with those advanced by Pratt ; see in particular Pratt's art. 192. It would occupy too much space to examine here the cause to which M. Faye attributes the greater density of the suboceanic crust.—O. F.

II. *On a Modification of* Wheatstone's *Rheostat.*
By SHELFORD BIDWELL, *M.A.*[*]

IT is frequently desirable that the resistance of a circuit through which a current of electricity is flowing should be made to vary *continuously* and not by steps, as is necessarily the case when resistance-coils are used alone. An instrument for effecting this object was devised by Sir Charles Wheatstone in 1843, and was called by him the Rheostat. Two forms of the apparatus, both of which are well known, are described in his paper on the " Constants of the Voltaic Circuit," originally published in the Phil. Trans. and contained in the Physical Society's Reprint of Wheatstone's Scientific Memoirs.

* Communicated by the Physical Society : read May 8, 1886.

In the first form two equal cylinders, one of wood and the other of brass, are mounted on parallel axes, and so arranged that a fine wire can be wound off the one and on to the other by turning a handle. On the wood cylinder a spiral groove is cut in which the wire lies, so that its successive convolutions are insulated from each other ; but when any portion of the wire is wound upon the brass cylinder, the current passes immediately from the wire to the cylinder, and thence to one of the terminals of the instrument. The effective part of the length of the wire is therefore the variable portion which is on the wood cylinder.

In the other form there is only one cylinder, which is of wood, and has a quantity of stout wire permanently wound upon it in a spiral groove cut upon its surface. Near the cylinder and parallel to its axis is a brass rod, upon which a notched "rider," or in more modern instruments a grooved brass wheel, is capable of sliding. The notch in the rider, or the groove in the wheel, fits and presses upon the spiral wire, and when the cylinder is turned the rider or wheel moves longitudinally along the brass rod. The terminals of the instrument are connected respectively with the brass rod and with one end of the wire upon the cylinder ; and the resistance introduced into the circuit depends upon the position of the point of contact of the rider or wheel with the wire.

The apparatus in either form is open to serious practical objections. In that first described it is difficult to maintain good electrical contact between the wire and the brass cylinder, both of which must be kept perfectly clean and free from dust and damp. Moreover the wire is liable to become slack and to leave its groove, and not unfrequently it breaks. In the second form there is also a difficulty in securing uniformly good contact ; and if the slider fits the rod sufficiently tight, the lateral pressure upon the wire is so great that it becomes permanently stretched, and is sometimes forced completely out of its groove. The wire, too, is necessarily thick; and the whole apparatus must consequently be large and cumbersome if it is to work through any considerable range of resistance.

A Wheatstone's rheostat in good working order is rarely seen, even in the shop of the instrument maker ; and in point of fact it is little used except for the purpose of lecture-illustration.

In the course of some experimental work the pressing need of some means of continuously varying a resistance led me to devise the modified rheostat, which is figured in the annexed woodcut.

As in the second form of Wheatstone's instrument, a wire

is coiled in a spiral groove upon an insulating cylinder. This is mounted upon the middle of a brass axle of rather more than three times its length. Upon one of the projecting ends of the axle a screw is cut, the pitch of which is equal to
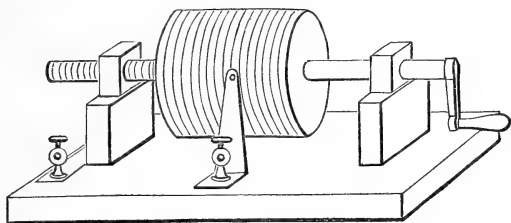
the distance between the consecutive turns of the wire. The axle revolves in two brass bearings, fixed at a distance apart equal to twice the length of the cylinder ; one of the bearings has an inside screw corresponding with that upon the axle. A flat spring is attached at one end to the base-board of the instrument midway between the bearings ; to the other end is riveted a short copper pin, which is directed perpendicularly to the axis of the cylinder and bears upon the spiral wire, being kept in position by a shallow notch cut in its end. One end of the spiral wire is electrically connected with the brass axle, and thence through the screwed bearing and a strip of copper with a terminal upon the base-board. The spring is directly connected with a second terminal. When the cylinder is turned by means of a handle, it travels backwards or forwards in the direction of its axis, the point of contact of the copper pin with the spiral wire remaining fixed in space ; thus more or less resistance is introduced between the axle and the spring, and therefore between the two terminals.

In the model exhibited at the meeting of the Physical Society, the length of the cylinder is 3 cm., its diameter is 8 cm., and the diameter of the wire, which is of German silver, is 0·5 mm. There are nine turns of wire per centimetre of length, and the greatest resistance is about 10 ohms. Since the instrument is generally used in conjunction with a set of coils, this resistance is for most purposes amply sufficient ; indeed I am inclined to think that it would be on the whole advantageous to use thicker wire and wind it in a coarser spiral, so that the total resistance might be slightly more than one ohm. The rheostat would then be used merely as a fine adjustment*.

* The best form of cylinder would, I think, be a hollow brass drum covered with a tightly fitting tube of ebonite, 2 or 3 mm. in thickness. Wood, even when well seasoned, is liable to shrink, and is more or less

The advantages which this apparatus appears to possess over the usual forms are obvious.   It is simple, compact, easily made, and not easily put out of order.   There is no lateral stress, and in consequence of the rubbing action the contact is always good.   The deflections of the needle of a "dead-beat" galvanometer in circuit with it are under perfect control, responding to the rotation of the cylinder with smoothness and regularity, and there is complete freedom from jerks and retrograde movements.   It is not indeed more suitable for use as a measuring instrument than the older forms ; but when it is desired to adjust a resistance to a nicety, or to cause a continuous variation of a current, it is of great utility.   If the interposed resistance is required to be known with accuracy, it should be measured by the bridge-method in the ordinary way.

III. *Some Thermodynamical Relations.*—PART IV.
By WILLIAM RAMSAY, *Ph.D., and* SYDNEY YOUNG, *D.Sc.**

SINCE the first portions of this research were published Professors Ayrton and Perry have criticised our work, and while we would thank them for the appreciative way in which they speak of our labours, we must differ entirely from their conclusion that "there is nothing further to be said about the four laws in question."   This conclusion rests on a complete misapprehension of the whole scope of our papers, and for the obvious reason that, while we have endeavoured by a method of what may be termed approximation to arrive at a workable plan of deducing from the known vapour-pressures of a substance like water the unknown vapour-pressures of any other substance with as little expenditure of experimental work as possible, and have in our research made the statement, which is only a very rough approximation to truth, that the ratios of the absolute temperatures of any two liquids at any given pressure are equal to the ratios at any other pressure, but have materially modified this statement, and asserted next, what is a very close approximation to truth, that when the statement given does not hold (as in by far the

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affected by moisture.   The depth of the groove should be about half the diameter of the wire.   The wire should be annealed, and should be wound on when it is warm.   The notch at the end of the copper pin may be very shallow, for if the position of the spring is properly adjusted the pin has little or no tendency to slip off the wire.
  * Communicated by the Physical Society : read May 8, 1886.

majority of cases), the relation of the ratios is expressed by the equation

$$R' = R + c(t' - t)$$

(where R′ is the ratio of the absolute temperatures of any two bodies at a given pressure; R the ratio at another pressure; *t* and *t′* the temperatures of one of the two bodies corresponding to those vapour-pressures; and *c* is a constant). Professors Ayrton and Perry have based all their reasoning on the first statement, which laid no claim to exactitude, and have informed us, what we already knew, that Dalton's law gives better results. The data given in our previous papers, however, show how nearly our generalization agrees with fact.

To refute the accusation of having performed useless labour would be really to repeat the substance of our previous papers; but the members of the Physical Society would hardly thank us were we to do so. There are, however, two or three remarks made by our critics which call for special reply.

1. It is suggested that our obvious course would have been to employ Rankine's formula, which, our critics state, is not empirical, but based on Rankine's molecular theory. We have been informed that the formula deduced by Rankine from his molecular theory was found by him not to agree with the experimental results; and that, in order to secure agreement, an empirical term was added. We would ask if this was not the case, and, if so, whether the whole formula is not thereby rendered empirical?

2. We are told that our labours might have been reduced by 75 per cent. had we recognized the fact that our four laws are identical. Now we do not anywhere state that our "laws" are absolute, in which case only would the identity necessarily hold; but indeed give methods for calculating their deviation from constancy. It is moreover desirable to test the truth of such statements in every possible way, inasmuch as different determinations by different observers are thereby introduced.

3. That it is by no means impossible to obtain with accuracy the specific volumes of saturated vapour will appear from memoirs on the thermal behaviour of ethyl alcohol and ether, which are in the hands of the Royal Society; indeed such measurements can be made at temperatures when a direct determination of L would be extremely difficult, if not impossible. To show the necessity of using determinations from all sources, we may point out that if Regnault's values of L and of $\frac{dp}{dt}$ for alcohol and for ether are employed, and the

vapour-density calculated by the thermodynamic equation

$$\frac{L}{s_1 - s_2} = \frac{dp}{dt} \cdot \frac{t}{J},$$

impossible values are obtained, unless it is supposed that chemical dissociation takes place, which is absurd.

4. We are told that the equation numbered (6) (p. 371 of the 'Proceedings'), $t = \alpha \psi(p)$, is identical with our four "laws;" and that "if (6) is true, then it follows that the ratio of the absolute temperatures of two vapours to one another at any pressure is the same as at any other pressure, or $\theta = \kappa t$ (7). In fact, then, we can test laws I., II., III., and IV. by simply testing (7); and as we find from Regnault and Rankine's formula that (7) is untrue, there is nothing further to be said about the four laws in question." We hope that no one has, like Professors Ayrton and Perry, so misunderstood our papers as to suppose that we imagine that "the ratio of the temperatures of two vapours at any pressure is always the same as at any other pressure." We do, indeed, notice that this statement holds with very close approximation for like bodies, such as chloro- and bromo-benzene; but the whole of the second and third parts of our memoir are devoted to an attempt, which we deem not unsuccessful, to give a method whereby the *deviation from the above statement can be estimated*.

The didactic remarks so kindly directed to us at the beginning of Messrs. Ayrton and Perry's critique, lead us to suppose that the great importance of such problems in Chemistry is not so generally known as we had imagined ; and we would cordially agree that the cooperative principle may here with advantage be applied, and physicist and chemist mutually assist each other.

The problem of dissociation has for long invited the attention of chemists. Many compounds decompose at a moderately high temperature, and their constituents reunite when the temperature is lowered. It is argued that, were it possible to attain a sufficiently high temperature, all the simpler compounds at least would exhibit the phenomena of dissociation.

From the nature of gases, it is to be expected that a study of the behaviour of dissociating gases would be the simplest way of arriving at a knowledge of the subject. An obvious criterion of the amount of dissociation is given by the vapour-density of the partially dissociated body ; for if the dissociating gas, and the simpler gases resulting from its dissociation, obey Boyle's and Gay-Lussac's laws, then it is easy to calculate

from the density of the gas the percentage number of molecules decomposed. But there is no means of knowing whether the dissociating substance, as such, does or does not obey these laws; hence it is evidently desirable to ascertain the behaviour of substances which do not dissociate as regards expansion and compressibility; so that some idea may be gained as to the influence exerted by molecular attraction between like molecules on the volume they occupy. An investigation of this nature is best carried out by ascertaining the behaviour of gases when they are becoming increasingly subject to molecular attraction, *i. e.* when they are approaching the condition of saturated vapours. The pressure of saturation is, under normal circumstances, identical with the vapour-pressure; hence the importance of a knowledge of this quantity. We have before remarked, that we have communicated to the Royal Society the results of experiments on alcohol and ether with this view.

Again, if the body exerting vapour-pressure be one capable of partial dissociation on passage from the solid or liquid to the gaseous state, its apparent vapour-pressure will really consist of its real vapour-pressure, and also of the pressure due to its dissociation into its constituents. It is evidently of importance to determine the latter independently of the former; and it is only by the employment of some law applicable to all bodies, and deduced from the behaviour of stable substances, that an idea can be formed of the pressure due to the second cause.

In the Philosophical Magazine for April 1886, p. 299, Professor Unwin also comments on our papers, and, unlike Professors Ayrton and Perry, has appreciated our motive in bringing forward the subject. We have to thank him for pointing out the relation of the ratio of the total heat of vaporization to the heat expended in external work, deduced from the second half of the thermodynamic equation. We agree with him that the approximate constancy there observed lies at the basis of all the relations found by us.

We have taken the liberty of applying his formula

$$\frac{t^{n+1}}{p} \times \frac{dp}{dt}$$

to alcohol, ether, and mercury, using the vapour-pressures determined by us over a wide range of temperatures in the first two cases; and for mercury, the results given in the Journ. Chem. Soc. 1886, p. 37.

We have taken the value of $(n+1)$ for alcohol as $2\cdot248$, for ether as $2\cdot23$, and for mercury as $2\cdot21$.

The values of the expression $\dfrac{t^{n+1}}{p}\cdot\dfrac{dp}{dt}$ at different pressures are as follows :—

Alcohol.				Ether.			
Press. millim.	Temp. abs.	$\dfrac{dp}{dt}$.	$\dfrac{t^{2\cdot248}}{p}\cdot\dfrac{dp}{dt}$	Press. millim.	Temp. abs.	$\dfrac{dp}{dt}$.	$\dfrac{t^{2\cdot23}}{p}\cdot\dfrac{dp}{dt}$.
23·733	283	1·516	20700	184·9	273	8·843	12950
540·91	343	22·750	20376	921·18	313	31·16	12393
1692·3	373	58·175	20721	2293·9	343	62·84	12341
2359·8	383	75·95	20592	7495·7	393	153·95	12156
5686·6	413	151·75	20226	13281	423	234·9	12715
14764	453	315·45	19931	21804	453	336·4	12921
22182	473	430·8	19966				
45519	513	763·7	20703				

Mercury.

Pressures, millim.	Temp. abs.	$\dfrac{dp}{dt}$.	$\dfrac{t^{2\cdot21}}{dt}\cdot\dfrac{dp}{dt}$.
10·0	457·30	0·350	26486
100·0	534·20	2·506	26743
200·0	563·44	4·470	26833
500·0	608·03	9·40	26703
1000·0	647·20	16·37	26697
3000·0	723·10	38·61	26811
5000·0	765·60	56·90	26904

With mercury, the value of n is much more nearly constant than if Regnault's numbers be employed.

It will be noticed that this number is much higher than that given by Professor Unwin ($0\cdot69$, while ours is $1\cdot21$); but it is worthy of remark, that if the values of n be calculated for the stable substances mentioned in our first paper, taking the highest and lowest pressures, and the corresponding values of $\dfrac{dp}{dt}$, the value of n varies between the narrow limits $1\cdot15$ and $1\cdot52$. Although we have not verified these values for intermediate pressures except in the examples already given, it may be of interest to reproduce them.

Substance.	Pressures from which n was calculated.	Value of n.
Carbon disulphide . . .	50 and 5000	1·147
Ethyl bromide	50 and 5000	1·316
Ethyl chloride	50 and 5000	1·243
Carbon tetrachloride . .	50 and 5000	1·326
Bromobenzene	50 and 800	1·475
Chlorobenzene	100 and 700	1·480
Chloroform	200 and 5000	1·376
Ethylene	10 and 400	1·235
Sulphur	300 and 3000	1·251
Bromo-naphthalene . .	150 and 800	1·196
Methyl salicylate . . .	50 and 800	1·517
Aniline	50 and 800	1·218
Methyl alcohol	50 and 5000	1·395

It was found impossible to obtain a constant value for n with dissociating substances when different pressures were employed.

The whole subject of vapour-pressure is one that cannot well be considered on its own merits, for they must evidently be intimately related to other physical properties; and although we have already accumulated data for alcohol and ether bearing on this subject, we prefer to reserve a general consideration of the question until an investigation, as regards the thermal behaviour of other bodies, on which we are at present engaged, shall have been finished.

PART V.*

IN the first two of the series of papers on "Some Thermodynamical Relations," published in the Philosophical Magazine (December 1885, January and February 1886), it was shown by one of us that the ratio of the absolute temperatures of nearly related bodies, such as chlorobenzene and bromobenzene, or ethyl chloride and ethyl bromide, is constant for equal vapour-pressures; and that a relation exists between the ratios of the absolute temperatures of all bodies which may be expressed by the equation

$$R' = R + c(t' - t),$$

where R is the ratio of the absolute temperatures of the two bodies corresponding to any vapour-pressure, the same for both; R′ is the ratio at any other vapour-pressure, again the same for both; c is a constant which may be 0 or a small + or − number; and t' and t are the temperatures of one of the two bodies corresponding to the two vapour-pressures.

* Communicated by the Physical Society: read May 22, 1886.

The determination of the vapour-pressures of a large series of
compound ethers by Schumann (Wiedemann's *Annalen*, 1881,
p. 40) affords an opportunity of studying the relations of a great
number of bodies of the same type ; and it appears that in this
case also the ratio of the absolute temperatures of any two of
the ethers is a constant at all pressures. The following table
shows the ratios of the absolute temperatures of all the ethers
to those of ethyl acetate at pressures of 200, 760, and 1300
millims. (The boiling-points at these pressures are given by
Schumann.) It will be seen that in every case the ratio is
very nearly constant at each pressure ; and the deviations
from constancy may well be due to the great difficulty of ob-
taining perfectly pure specimens, evidenced by the fact that
only one ether boiled with perfect constancy, the rise of tem-
perature during distillation amounting in one case to $0°\cdot7$,
while the average rise was $0°\cdot37$. The absolute temperatures
of ethyl acetate corresponding to 1300, 760, and 200 millim.
are $367°\cdot3$, $350°\cdot1$, and $314°\cdot4$.

Ratios of Absolute Temperatures of Ethers to those of Ethyl
Acetate at definite Pressures.

Ether.	Pressure.			Mean.
	1300 millim.	760 millim.	200 millim.	
Methyl formate...............	·8715	·8720	·8706	·8714
Ethyl formate	·9344	·9352	·9323	·9340
Propyl formate...............	1·0109	1·0111	1·0115	1·0112
Isobutyl formate	1·0585	1·0594	1·0573	1·0584
Amyl formate	1·1345	1·1320	1·1329	1·1331
Methyl acetate	·9420	·9440	·9431	1·9430
Propyl acetate	1·0678	1·0677	1·0690	1·0682
Isobutyl acetate...............	1·1122	1·1120	1·1120	1·1121
Methyl propionate	1·0073	1·0080	1·0073	1·0075
Ethyl propionate	1·0615	1·0605	1·0614	1·0611
Propyl propionate	1·1290	1·1288	1·1291	1·1290
Isobutyl propionate	1·1704	1·1705	1·1705	1·1705
Amyl propionate	1·2366	1·2374	1·2401	1·2380
Methyl butyrate	1·0724	1·0720	1·0716	1·0720
Ethyl butyrate	1·1220	1·1223	1·1202	1·1215
Propyl butyrate	1·1889	1·1874	1·1902	1·1888
Isobutyl butyrate	1·2279	1·2279	1·2278	1·2279
Amyl butyrate	1·2905	1·2899	1·2901	1·2902
Methyl isobutyrate	1·0444	1·0434	1·0433	1·0437
Ethyl isobutyrate............	1·0953	1·0943	1·0935	1·0944
Propyl isobutyrate	1·1604	1·1623	1·1609	1·1612
Isobutyl isobutyrate	1·2001	1·1985	1·1985	1·1990
Amyl isobutyrate	1·2644	1·2619	1·2637	1·2633
Methyl valerate	1·1135	1·1131	1·1139	1·1135
Ethyl valerate	1·1642	1·1634	1·1619	1·1632
Propyl valerate...............	1·2257	1·2251	1·2265	1·2258
Isobutyl valerate	1·2635	1·2617	1·2634	1·2629

Taking the mean ratio for each ether, and the temperatures of ethyl acetate at the three pressures as correct, the boiling-point of the ethers were recalculated; and the following table contains these values, together with the observed temperatures and the differences between the observed and calculated temperatures :—

Absolute Temperatures.

Ether.	Observed.			Calculated.			Differences.		
	1300 mm.	760 mm.	200 mm.	1300 mm.	760 mm.	200 mm.	1300 mm.	760 mm.	200 mm.
Methyl formate......	320·1	305·3	273·7	320·1	305·1	274·0	0	−0·2	+0·3
Ethyl formate	343·2	327·4	293·1	343·1	327·0	293·6	−0·1	−0·4	+0·5
Propyl formate	371·3	354·0	318·0	371·4	354·0	317·9	+0·1	0	−0·1
Isobutyl formate ...	388·8	370·9	332·4	388·7	370·5	332·8	−0·1	−0·4	+0·4
Amyl formate	416·7	396·3	356·2	416·2	396·7	356·2	−0·5	+0·4	0
Methyl acetate	346·0	330·5	296·5	346·4	330·0	296·5	+0·4	−0·5	0
Propyl acetate	392·2	373·8	336·1	392·3	374·0	335·8	+0·1	+0·2	−0·3
Isobutyl acetate ...	408·5	389·3	349·6	408·5	389·3	349·6	0	0	0
Methyl propionate ..	370·0	352·9	316·7	370·1	352·7	316·8	+0·1	−0·2	+0·1
Ethyl propionate ...	389·9	371·3	333·7	389·7	371·5	333·6	−0·2	+0·2	−0·1
Propyl propionate...	414·7	395·2	355·0	414·7	395·3	355·0	0	+0·1	0
Isobutyl propionate	429·9	409·8	368·0	429·9	409·8	368·0	0	0	0
Amyl propionate ...	454·2	433·2	389·9	454·7	433·4	389·2	+0·5	+0·2	−0·7
Methyl butyrate ...	393·9	375·3	336·9	393·7	375·3	337·0	−0·2	0	+0·1
Ethyl butyrate	412·1	392·9	352·2	411·9	392·6	352·6	−0·2	−0·3	+0·4
Propyl butyrate ...	436·7	415·7	374·2	436·6	416·2	373·8	−0·1	+0·5	−0·4
Isobutyl butyrate ...	451·0	429·9	386·0	451·0	429·9	386·0	0	0	0
Amyl butyrate	474·0	451·6	405·6	473·9	451·7	405·6	−0·1	+0·1	0
Methyl isobutyrate..	383·6	365·3	328·0	383·4	365·4	328·1	−0·2	+0·1	+0·1
Ethyl isobutyrate...	402·3	383·1	344·1	402·0	383·2	344·1	−0·3	+0·1	0
Propyl isobutyrate..	426·2	406·9	365·0	426·5	406·5	365·1	+0·3	−0·4	+0·1
Isobutyl isobutyrate	440·8	419·6	376·8	440·4	419·8	377·0	−0·4	+0·2	+0·2
Amyl isobutyrate ...	464·4	441·8	397·3	464·0	442·3	397·2	−0·4	+0·5	−0·1
Methyl valerate......	409·0	389·7	350·2	409·0	389·8	350·1	0	+0·1	−0·1
Ethyl valerate	427·6	407·3	365·3	427·2	407·2	365·7	−0·4	−0·1	+0·4
Propyl valerate......	450·2	428·9	385·6	450·2	429·2	385·4	0	+0·3	−0·2
Isobutyl valerate ...	464·1	441·7	397·2	463·9	442·1	397·1	−0·2	+0·4	−0·1

It will be noticed that the greatest difference between the observed and recalculated temperatures is $0°\cdot7$, which is probably within the limits of experimental error, for the reason already stated. If this is so, the equation $\dfrac{t_2'}{t_1'} = \dfrac{t_2}{t_1}$ holds good for this series of ethers, where t_2' and t_2 are the absolute temperatures of any one of the ethers corresponding to two vapour-pressures, and t_1' and t_1 the absolute temperatures of any other ether at the same pressures. If the ethers are compared with some other substance, such as water, the equation

$$\frac{t_1'}{T'} = \frac{t_1}{T} + c(T' - T)$$

must be employed, where t_1' and t_1 are the absolute tempera-
tures of the ether, and T′ and T those of water at any two
pressures. If the left side of this equation be multiplied by
$\dfrac{t_2'}{t_1'}$ and the right by $\dfrac{t_2}{t_1}$, $\dfrac{t_2'}{t_1'}$ being equal to $\dfrac{t_2}{t_1}$, the equation
becomes

$$\frac{t_2'}{T'} = \frac{t_2}{t_1}\left\{\frac{t_1}{T} + c(T'-T)\right\}$$

$$= \frac{t_2}{T} + c\frac{t_2}{t_1}(T'-T).$$

That is to say, if the members of this series of ethers be com-
pared with water, both the ratios at any given pressure,
corresponding to t_1, t_2, &c. and the constant c vary directly
as the boiling-points (absolute) of the ethers at that pressure.

Taking the boiling-point of ethyl acetate at the normal
pressure to be correct, and the ratio of the absolute tempera-
ture of ethyl acetate to that of water at that pressure therefore
to be $\dfrac{350\cdot 1}{373\cdot 0} = 0\cdot 9386$, and the value of c to be $0\cdot 000387$, the
ratio of the absolute temperature of ethyl acetate to that of
water at any other pressure is given by the equation

$$\frac{t'}{T'} = 0\cdot 9386 + 0\cdot 000387\,(T'-373),$$

where t' is the absolute temperature of ethyl acetate, and T′
that of water.

The calculated and observed temperatures are :—

	Temperature.	
Pressure.	Observed.	Calculated.
1300	367·3	367·2
200	314·4	314·4

For any other ether in the series the equation is

$$\frac{t'}{T'} = \frac{t}{350\cdot 1}\left\{0\cdot 9386 + 0\cdot 000387\,(T'-373)\right\},$$

where t is the boiling-point of the ether at constant pressure.

For pressures of 1300 and 200 millim. respectively the
equation becomes simply

$$t' = 1\cdot 0488\,t$$

and

$$t' = 0\cdot 89795\,t.$$

It appears therefore that by determining simply the boiling-
point of an ether belonging to this type, and knowing the
vapour-pressures of water (or of one of the other ethers), it is
possible to calculate the temperatures corresponding to any
other pressure, at any rate between the limits of 200 and
1300 millim.

IV. *On a new Method for the Preparation of Tin Tetrethyl.*
By Prof. E. A. LETTS, *D.Sc., Ph.D., &c., Queen's College,*
Belfast, and NORMAN COLLIE, *Ph.D., Science Lecturer, the*
Ladies College, Cheltenham[*].

HAVING had occasion to prepare large quantities of zinc
ethyl, we employed Gladstone and Tribe's excellent
method[†], and observed quite accidentally the formation of a
liquid bye-product which contained a metal, but was not
affected by air nor decomposed by water.

For some time we were baffled in our attempts to ascer-
tain the nature of this substance, as we scarcely anticipated
that the commercial zinc we employed would contain any
foreign metal in sufficient quantity to give an organo-metallic
body. Eventually, however, we proved that the substance
contained tin, and was in fact tin tetrethyl, as the following
particulars show.

Boiling-point 179°–180° C.

(1) 0·232 grm. substance gave 0·3465 grm. CO_2 and
0·1820 grm. H_2O.

(2) 0·244 grm. substance gave 0·3655 grm. CO_2 and
0·1925 grm. H_2O.

	I.	II.
Carbon	40·73	40·91
Hydrogen . . .	8·71	8·77

And a vapour-density determination carried out by Hofmann's
method gave the following numbers :—

	I.	II.
Weight of substance taken .	0·1019 grm.	0·02568 grm.
Temperature of vapour . .	182° C.	182° C.
Height of barometer . . .	761 mm.	762·5 mm.
Height of mercury in tube .	504 mm.	344 mm.
Temperature of air	15° C.	15° C.

	I.	II.
Vapour-density . . .	8·16	7·96

0·822 grm. substance gave 0·532 $SnO_2 = 50·04$ per cent. Sn.

	Calculated for $(C_2H_5)_4Sn$.	Found.		
		I.	II.	III.
Carbon . . .	41·02	40·73	40·91	...
Hydrogen . .	8·54	8·71	8·77	...
Tin	50·43	...	...	50·04

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[*] Communicated by the Authors.
[†] Chem. Soc. Journ. 1879, i. p. 571.

Vapour-density of Tin tetrethyl. Calculated.	Found.	
	I.	II.
8·09 . . .	8·16	7·96

As the usual methods for the preparation of tin tetrethyl are rather troublesome and tedious (viz. either by the action of zinc ethyl on tin tetrachloride*, or on tin triethyl iodide, or on tin diethyl iodide †, or by adding fused anhydrous stannous chloride to zinc ethyl and distilling‡), it occurred to us that the accidental discovery we have described might indicate a simpler and more satisfactory process for obtaining it.   As the commercial zinc we employed could only contain a small percentage of tin, it seemed probable (as we had obtained fair quantities of the tetrethyl compound) that the whole of the tin had possibly been converted into its organo-metallic derivative ; and we argued that, by employing an alloy of zinc rich in tin for the couple, or by using pure tin without admixture of zinc, we ought to obtain a good yield of tin tetrethyl.

A rough experiment showed that tin tetrethyl is obtained in considerable quantity by employing a couple containing a fair percentage of tin, and that the process is easily executed.

We next made a series of experiments to ascertain the conditions for obtaining the best yield of the organo-metallic body, and also for ascertaining what proportion of tin, in alloys with zinc, of varying composition would be converted into the tetrethyl compound.   The following table illustrates our results :—

Preparation of Tin Tetrethyl.

Exp.	Couple used = 50 grms., containing 45 grms. alloy and 5 grms. Cu.				Ethyl iodide.	Tin tetrethyl obtained.	Per cent. of Tin converted into Tin tetrethyl.
	Alloy.			Copper.			
	Zn.	Sn.	Per cent. Sn.				
1. ...	44·45	0·45	1·0	5	60	0·5	55
2. ...	43·87	1·13	2·5	5	60	1·0	45
3. ...	42·75	2·25	5·0	5	60	2·5	56
4. ...	40·50	4·50	10·0	5	60	4·0	45
5. ...	38·25	6·75	15·0	5	60	5·0	37
6. ...	36·00	9·00	20·0	5	60	10·0	56
7. ...	30·00	15·00	33·3	5	60	12·0	40
8. ...	22·50	22·50	50·0	5	60	10·0	22

* Buckton, Phil. Trans. 1859, p. 426.   † Ibid. p. 424.
‡ Frankland and Lawrance, Chem. Soc. Journ. 1879, i. p. 130.

These results indicate that, with the quantity of alloy used (viz. 45 grms.), about 50 per cent. of the tin is always converted into tin tetrethyl, no matter what the composition of the alloy may be, *so long as the proportion of tin does not exceed from* 20 *to* 60 *per cent.* They also show that the total quantity of tin tetrethyl obtained is proportional to the percentage of tin in the alloy up to about 33·3 per cent., but not beyond; possibly for the following reason:—A couple composed of tin and copper only has no immediate action on ethyl iodide; but a zinc-copper couple is very active, giving zinc ethiodide almost immediately. The production of this substance appears to be a necessary preliminary to the formation of tin tetrethyl; and therefore as the percentage of zinc is reduced, the formation of the zinc ethiodide is retarded, if not interfered with altogether.

Up to a certain point, then, sufficient zinc ethiodide is formed to induce the reaction; but if the percentage is further increased, the action seems to stop, or at least to be hindered. The following table illustrates this point. In each experiment 60 grms. of ethyl iodide was allowed to act on 50 grms. of couple.

Experiment.	Percentage of Tin in alloy.	Reaction finished, after commencement*, in
1.	1	10 minutes.
2.	$2\frac{1}{2}$	20 ,,
3.	5	30 ,,
4.	10	40 ,,
5.	15	30 ,,
6.	20	40 ,,
7.	33·3	240 ,,
8.	50	480[1] ,,

[1] In experiment 8 the digestion was stopped at the end of eight hours, although the reaction was by no means finished; for on distillation 24 grms. of unacted-on ethyl iodide were recovered.

From experiments 7 and 8, it is evident that the time necessary for the action increases enormously with high percentages of tin in the alloy.

* The commencement of the action of ethyl iodide on the couple is always indicated by an apparent effervescence in place of the simple ebullition which occurs at first.

As regards the mechanism of the reaction, the following explanation may be suggested :—

$$2Zn(C_2H_5)I + Sn = Sn(C_2H_5)_2 + ZnI_2 + Zn,$$

or

$$Zn(C_2H_5)_2 + Sn = Sn(C_2H_5)_2 + Zn,$$

and

$$2Sn(C_2H_5)_2 = Sn + Sn(C_2H_5)_4.$$
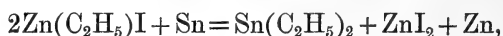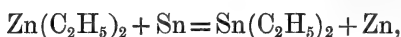
And this explains the fact that about 50 per cent. only of the tin is converted into tin tetrethyl. If the above explanation be true, an *alloy* of zinc and tin might not be necessary; and also if the conversion of the tin into tin tetrethyl takes place at the moment the zinc ethiodide is decomposed by heat, the action of the ethyl iodide on the couple is only to produce zinc ethiodide. Several other experiments were therefore made in order to ascertain as far as possible whether this is the case.

A couple was made of ordinary zinc and copper, to which ethyl iodide and powdered tin were added.

Couple used = 40 grm.		Powdered Tin.	Ethyl iodide.	Action began in	Tin tetrethyl produced.
Zn.	Cu.				
36	4	20 grm.	60 grm.	15 min.	3 grm.

This proves beyond doubt that an alloy of the zinc and tin is not absolutely necessary.

In the next experiment, after the whole of the ethyl iodide had been converted into zinc ethiodide by an alloy containing 15 per cent. of tin, the flask connected with an upright condenser was heated in an oil-bath for about three hours to a temperature high enough to decompose the zinc ethiodide, and so allow it every facility for reacting on the tin.

Couple used = 66 grm.			Copper.	Ethyl iodide.	Finished in	Tin tetrethyl produced.	Per cent. of Tin converted into $Sn(C_2H_5)_4$.
Alloy.							
Zn.	Sn.	Per cent. Sn.					
51	9	15	6 grm.	60 grm.	20 min.	11 grm.	61 per cent.

Ethyl iodide was also allowed to act on some zinc-tin alloy

containing 15 per cent. of tin. When the reaction was finished, the resulting crystalline mass was dissolved in ether and the zinc ethiodide decomposed by the careful addition of alcohol, and eventually acidulated water. Only traces of tin tetrethyl could be detected in the etherial solution, while in the watery solution no tin compounds were present. This result indicates that at the heat of the water-bath, zinc ethiodide does not act upon tin.

Another experiment was therefore tried to see whether, by digesting zinc ethiodide with tin at the temperature at which the former substance decomposes, the formation of tin tetrethyl would take place readily. Fused zinc ethiodide was poured on to about half its own weight of powdered tin, and then digested in an oil-bath for three hours at a temperature of about 160° C. On distilling, the liquid which passed over scarcely even fumed in the air, and was nearly pure tin tetrethyl.

Although we have not ascertained the exact conditions necessary for obtaining the maximum yield of tin tetrethyl, yet our last experiment would seem to show that these conditions would be realized by first obtaining a maximum yield of zinc ethiodide, by acting on a zinc-copper couple with ethyl iodide, then adding powdered tin and digesting at 150°–160° C. until all action is over, and finally distilling.

The action of ethyl iodide on alloys of zinc containing other metals was tried; alloys of 10 per cent. of zinc with 20 per cent. of bismuth, aluminium, antimony, and lead respectively were treated with ethyl iodide, but with negative results. The alloy containing 20 per cent. of bismuth was digested on the water-bath for several hours. After all the ethyl iodide had been converted into zinc ethiodide, further heating was considered unnecesary, as bismuth triethyl decomposes at 50°–60° C. The solid mass was extracted with dry ether when cool, and filtered; alcohol was first added and then acidulated water, but not even a trace of an organo-metallic bismuth compound could be detected.

The aluminium alloy also did not yield even a trace of any volatile organic aluminium compound.

The lead alloy, after the whole of the ethyl iodide was converted into the zinc ethiodide, was kept for several hours at a temperature 160°–170° C.; but on allowing it to cool and extracting with dry ether no lead compound was dissolved.

The antimony alloy gave traces of a volatile antimony compound which distilled with the zinc ethyl, but in such small quantity that the investigation was not carried further.

It is remarkable that, while alloys of zinc and the above-

mentioned metals yield with iodide of ethyl only zinc ethyl,
a tin alloy should give such large quantities of tin tetrethyl.
This may be accounted for perhaps by the greater stability
of the tin tetrethyl at high temperatures, the organo-metallic
compounds of bismuth, lead, aluminium, and antimony all
decomposing when strongly heated.

V. *Note on* M. Mascart's *paper, " On Magnetization."* *
By E. F. J. Love, *B.A., Demonstrator of Physics in the
Mason Science College, Birmingham* †.

IN the above-mentioned paper, M. Mascart draws attention
to the fact that the values of the coefficient of magnet-
ization, as determined by using long cylinders, differ from
those obtained by the employment of rings ; and tries to
demonstrate that " a special phenomenon is produced in the
case of closed rings which exaggerates the effects of induc-
tion."

Without presuming to question M. Mascart's investigations,
I may be allowed to point out that the facts admit of an
entirely different and much simpler explanation.

Calling the two values of the magnetization-coefficient
determined with the help of bars, f and f_1, as is done by
M. Mascart, I may first point out that f_1 is measured by
finding the magnetic induction at the centre of the bar, while
f is determined by the magnetic moment, which depends on
the distribution of magnetic induction throughout the bar.
In all cases the induction through the central section is the
greatest, owing to lines of induction leaving the bar all along
its length, and not all going right through to the ends ; the
effect of this is to diminish the apparent value of the magneti-
zation-coefficient as determined from the magnetic moment.
The extent to which the induction-lines escape is very
surprising, as the following example will show :—

A rectangular bar of soft iron was bent into a ring, and
turned in the lathe to a true circle, 10 centim. in diameter.
It was then cut into two halves along a diameter, and the cut
ends worked to true planes. On each half a primary coil of 185
turns was wound, and these were connected together, giving a
primary of 370 turns covering up the iron.   Two secondary
coils of 10 turns each were then wound on, one near the end
of one of the semicircles, the other forming a spiral extending
over the surface of the same semicircle.   The two electro-

magnets thus formed were placed on a block between springs which held their ends firmly in contact, thus giving a closed iron circuit. A magnetizing current being passed through the primary, the induction through the secondary coils on reversing the primary current was measured, and found to be the same for both coils. Then a piece of copper 0·5 millim. thick was interposed between each of the opposed pairs of faces of iron ; and the induction near the end was found to be only 0·73 of the mean induction.

The effect produced with a long iron rod would naturally be of the same kind as with the ring described above, except that the leakage of induction would take place to a greater extent.

Another point is, that in experiments with a closed iron ring the lines of induction lie entirely within the iron, and hence the magnetic permeability μ $(=1+4\pi k)$ is determined directly ; but when a rod is employed, the lines of induction pass for the greatest part of their course through air, and what is measured is the *average* permeability of an air-iron circuit. Now air having always a smaller permeability than iron, the result is to bring out the magnetization-coefficient again too low.

Hence it would hardly seem necessary to adopt M. Mascart's suggestion that " a special phenomenon is produced in the case of closed rings which exaggerates the effect of induction."

VI. *On a Theorem relating to Curved Diffraction-gratings.* By WALTER BAILY, *M.A.*[*]

IN a paper read before this Society in January 1883 [†], I showed that if a plane be taken perpendicular to the lines of a curved diffraction-grating, and the normal to the centre of the grating be taken as the initial line, then the equation

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

in which c is the radius of curvature of the grating and d is an arbitrary constant, gives a curve having the property that if a source of light is placed at any point of the curve, the curve is the locus of the foci of all diffracted rays whether reflected or transmitted.

When d is greater than c, r may be infinite. Let ϕ be the

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value of θ for which r is infinite.   Then

$$0 = \frac{\cos\phi}{c} + \frac{1}{d},$$

and the equation to the curve may be written

$$\frac{c}{r} \cdot \cos^2\theta = \cos\theta - \cos\phi. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In fig. 1, G is the diffraction-grating and G N its normal ;

Fig. 1.
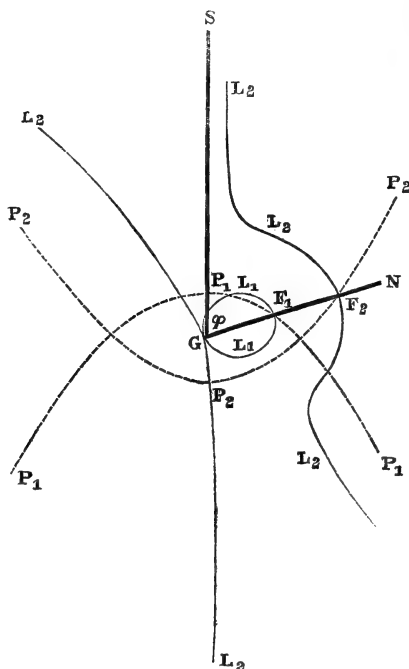
and the angle N G S is equal to ϕ.   The curves marked L_1, L_2
are traced from equation (2). G S is parallel to one asymptote
to the curve, so that if a source of light (say a star) is at an
infinite distance on the corresponding branch of the curve it
lies in the direction G S ; and the foci of its diffracted light
lie on the curves L_1, L_2.   The foci of reflected light will lie on
the oval marked L_1, and the foci of transmitted light will lie
on the branches marked L_2.

Let F_1, F_2 be the points at which these curves cut the
normal to the grating, and let $GF_1 = \rho_1$ and $GF_2 = \rho_2$.   The
points F_1, F_2 I will call the normal foci ; F_1 is the normal
focus for reflected light, and F_2 for transmitted light.

Putting $r = \rho_1$ and $\theta = 0$ in (2), we have

$$c/\rho_1 = 1 - \cos\phi; \quad \cdots \quad \cdots \quad (3)$$

and, again, putting $-r = \rho_2$ and $\theta = \pi$, we have

$$c/\rho_2 = 1 + \cos\phi. \quad \cdots \quad \cdots \quad (4)$$

Now suppose G S the direction of the star to be kept fixed, and the grating turned so as to vary the value of ϕ; then equations (3) and (4) will give the loci of F_1 and F_2 respectively. The loci are the parabolas marked P_1 and P_2. They have a common focus G, and common axis GS, and a common latus rectum $= 2c$.

F_1 is the real focus of reflected light, having a wave-length $= \dfrac{\sigma}{n}\sin\phi$, where σ is the distance between the lines of the grating, and n is an integer.

F_2 is the virtual focus of transmitted light having the same wave-length.

In fig. 2, P P is part of the parabola (on a larger scale)

Fig. 2.
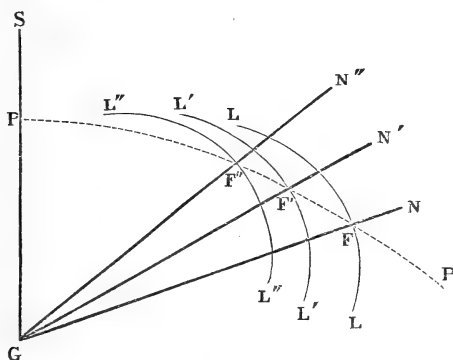
which is the locus of the normal focus of reflected light; G N, G N', G N'' are difference positions of the normal to the grating ; and L L, L' L', L'' L'' are respective positions of the spectra, and F, F', F'' are respective positions of the normal foci.

The theorem is, When the sources of light are at an infinite distance the normal foci lie on two parabolas whose common focus is the centre of the grating and common latus rectum is equal to the diameter of curvature of the grating ; the parabolas for reflected light being convex to the source of light, and those for transmitted light being concave.

VII. *Magnetic Researches.* *By* Prof. G. WIEDEMANN[*].

[Plate I.]

SINCE the year 1857 I have published [†] a series of inves-
tigations on the relationship between the mechanical and
magnetic properties of bodies, and in 1877 a closely-related
paper[‡] on Torsion, in which I proposed to make, in continua-
tion, observations on magnetism. In the mean time other
investigators have made researches upon the same subject,
which indeed are partly only repetitions and extensions of
previous work in slightly different form ; but partly also have
constituted an actual advance, and in particular points have
been supposed to be in opposition to the theory I have pro-
posed. I may therefore be permitted briefly to mention these
researches here, and to describe certain new experiments
connected with them.

§ 1.

Mr. Hughes[§] has investigated the change in magnetic
moment of iron and steel, upon torsion &c., under various
conditions, as Wertheim and Matteuci had already done by
means of the currents induced in a spiral surrounding a
magnetic body.

He has given a series of experiments on the behaviour,
during torsion, of iron wires through or round which a current
is passed. I have already[||], in a short note published in
English, called attention to the fact that the essential portion
of these results had been already observed ·by myself in the
years 1858–60. Nevertheless, Mr. Hughes in further com-
munications[¶], after a not altogether correct or quite complete

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* Translated from Wiedemann's *Annalen* for March 1886.

† G. Wiedemann, *Verhandl. der Baseler Naturf. Ges.* ii. p. 169. Pogg.
Ann. c. p. 235 (1857); ciii. p. 161 (1858) ; cvi. p. 169 (1859); cxvii.
p. 195 (1862). Collected in Wiedemann's *Galvanismus* (2nd ed.) and
Electricität (3rd ed.).

‡ G. Wiedemann, Wied. *Ann.* vi. p. 485 (1879).

§ Proc. Roy. Soc. Lond. xxxi. p. 525 (1879), xxxii. pp. 25, 213 (1881) ;
Beibl. v. pp. 538, 687 (1881).

|| G. Wiedemann, Phil. Mag. (5) xii. p. 223 (1881); *Beibl.* v. p. 689
(1881).

¶ Hughes, Chem. News, xlvii. p. 63 (1883) ; Nature, xxviii. pp. 159,
183 (1883); see also xxix. p. 459 (1884) ; Inst. of Mechan. Engineers,
1884 ; Proc. Roy. Soc. Lond. xxxv. pp. 19, 178 (1882), xxxvi. p. 405
(1884) ; Journ. Soc. Telegr. Engineers, xii. p. 374 (1883) ; Theory of
Magnetism, Royal Inst. Lecture, February 29, 1884.

citation* of earlier work, has (together with certain experiments following at once from my views) reproduced my theory as new. This theory has thus, in various quarters, been supposed to have originated with him.

Mr. Hughes passes, for instance, alternating currents through a flat iron band, surrounds the middle of this with an induction-spiral, and, in a telephone connected with the latter, hears no sounds; as was to be expected, since, with both directions of the current, the potential of the transverse and circularly-directed molecular magnets upon the spiral is zero. But if the iron band be twisted meanwhile, then, as I have expressly explained, the transversal molecular magnets take up a position inclined to the axis of the band, and partially maintain this position during the detorsion. Hence, under the influence of the alternating currents, they assume positions variously inclined to the axis, so that the potentials can no longer be zero, or equal to each other; hence the telephone sounds. That upon the sudden springing back of the twisted band the molecular magnets may spring over to the other side, and that then the induced currents run in the opposite direction, is clear. It is equally obvious that the molecules of the iron tape may take up an inclined position upon the lateral approach of a magnet, though not spirally arranged round the axis, and that similar phenomena must result. These conclusions follow at once from my earlier results.

In connexion with these and similar experiments, Mr. Hughes proposes the theory (*a*) that each molecule of a magnetic metal is an unalterable molecular magnet; (*b*) that it can be caused to revolve on its axis by means of mechanical and electromechanical forces; (*c*) that when there is external magnetic neutrality, the molecules and their polarities are so arranged that they satisfy their mutual attractions in the shortest possible way; and, lastly, (*d*) that if magnetism is produced, the molecules and their polarities become rotated into a given direction in a symmetrical manner, so that a north pole is produced in the one direction and a south pole in the opposite direction. Moreover, also, a symmetrical arrangement is assumed when magnetism is produced, but the circuits of attraction are not complete unless there is a keeper connecting the two poles. Hence Mr. Hughes assumes still further (*e*) a

* Thus, amongst others, the theory of rotation of molecular magnets, given in De la Rive's *Traité d'Électricité,* English edition, Sect. iii. p. 317 (1853), is attributed to De la Rive; the much earlier fundamental deduction of W. Weber, *Electrodyn. Maasbest.* iii. p. 557 (1846) is not mentioned. My conclusions are not quite correctly represented.

rigidity, which retains the molecules in their positions after the cessation of the active forces.

We have long been acquainted with all these propositions. They agree perfectly, so far as they are correct, with those which I have published after the earlier calculations of Weber; as moreover is recognized by Prof. G. Hoffmann, the reporter upon Mr. Hughes's experiments in the 'Electrotechnical Journal'[*]. It is only in regard to proposition (*c*) that he thinks Mr. Hughes has the priority. This is, however, not correct. In the second edition of my *Galvanismus,* ii. (1) p. 373, § 327 (1871) I say distinctly:—" It is obvious that we may imagine an infinite number of arrangements of the molecular magnets in bodies, with each of which no external action would result. If, for example, they are arranged in circles with their unlike poles in contact, or if their poles have in each space-element all possible directions, this result will follow." The assumption of Mr. Hughes, that I have referred the neutrality of a bar to a "heterogeneous" arrangement of molecules, which is completely different from his theory of neutrality, is thus opposed to facts.

Moreover, it is incorrect that the external magnetic neutrality, whether of a new bar or in one subject to magnetizing influences, is conditioned solely by the formation of closed molecular rings, such as Mr. Hughes imagines. This would be possible only with a power of perfectly free rotation of the molecules round their centres of gravity. But since experience shows that they suffer a resistance to this rotation, this arrangement can only be approximately attained; the axes of the molecular magnets must in like manner deviate from the axis of such a ring on all sides. Moreover there is, *à priori*, no ground why any one direction should have the preference in an absolutely neutral magnet, either for the position of the separate molecular magnets, or for the rings approximately formed by their opposite attractions. The second statement of my proposition quoted above is therefore entirely justified.

Lastly, Mr. Hughes's proposition (*d*), that in visible magnetization a "symmetrical" rotation and arrangement of the molecular magnets takes place, is only correct in the case of a symmetrical form of the body magnetized, and a symmetrical distribution of the magnetizing forces.

§ 2.

Sir William Thomson[†] has endeavoured to explain the

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[*] G. Hoffmann, *Electrotechnische Zeitschrift,* iv. p. 366 (1883).
[†] Proc. Roy. Soc. Lond. xvii. p. 442 (1878).

experiments which I made in the year 1858—showing that an iron or a steel bar, subjected to torsion during or after passing a galvanic current through it, becomes magnetic— by assuming that the fibres of the bar assume a spiral arrangement upon torsion, and that so that the current also circulates spirally through it. At the same time he himself mentions the difficulty that the effect of torsion is constant in direction up to the strongest current which can be passed through the wire, whilst aelotropy would result only with feeble magnetization, and moreover is opposed to the action of aelotropy upon the electric conductivity.

The above view is also opposed by the fact that, upon stopping the current through the wire, wires which have thus acquired permanent transversal-circular magnetism have produced in them by torsion a permanent axial magnetism. Moreover the opposite behaviour of nickel and iron (see below) contradicts this assumption[*]. But that this cause may have some secondary action at the same time as the rotation of the molecular magnets is hardly to be doubted.

§ 3.

According to the experiments of Sir W. Thomson[†], the behaviour of magnetic bars of iron, on the one hand, is opposed to that of magnetic bars of nickel and cobalt, on the other hand, when subjected to longitudinal tension. Whilst the temporary magnetism of the first increases with the load under a certain critical value of the magnetic force, above which it decreases, the temporary magnetism of nickel and cobalt decreases below a certain other (much larger) critical value of the magnetizing force.

Reciprocally, it follows from the experiments of Barret[‡], that whilst (as Joule § has shown) iron bars lengthen upon being magnetized, bars of nickel, on the contrary, shorten. Cobalt is reported to form an exception and to behave like iron. In these latter experiments, it is true, the purely mechanico-electromagnetic action of the magnetizing coil upon the bar was not excluded.

Quite in agreement with this, Mr. Knott ‖ has found that when a current is passed through a bar of nickel magnetized

* Compare also Ewing's proposal of an experiment on this point, Phil. Mag. (5) xiii. p. 423 (1882); *Beibl.* vi. p. 809 (1882).

† Proc. Roy. Soc. xxiii. pp. 445, 473 (1875); xxvii. p. 442 (1878) Phil. Trans. clxvi. (2) p. 693 (1877); *Beibl.* ii. pp. 362, 607 (1878).

‡ Phil. Mag. (4) xlvii. p. 51 (1874); *Beibl.* vii. p. 201 (1883).

§ Phil. Mag. xxx. p. 76, 225 (1847).

‖ Proc. Roy. Soc. Edinb. 1882–83, p. 225; *Beibl.* viii. p. 399 (1884).

longitudinally, it suffers torsion in a direction opposite to that which I found for an iron bar treated in the same way.

Maxwell* and Chrystal† have formerly endeavoured to refer this torsion, in the case of iron, to the above-mentioned expansion which it suffers upon being magnetized. Wires magnetized at the same time longitudinally and transversely, and therefore in an obliquely spiral direction, ought to expand in this latter direction, and thus to suffer torsion in the direction determined. But with nickel, since, upon being magnetized, contraction instead of expansion takes place, the torsion ought to take place in the opposite direction, as is shown by experiment to be the case.

I have myself endeavoured to refer this phenomenon, observed in my first experiments in 1858, at least in the case of iron wires, to the obliquely spiral direction which the molecules take up in consequence of the two magnetizations at right angles to each other, and which is accompanied secondarily by a displacement of the longitudinal fibres and sections of the wires, of which, in the case of iron, the first is the most important.

The chief proof of my assumption which I had given was the phenomenon, completely coordinate with the above results, that a wire equatorially magnetized, whether temporarily or permanently, by a current passed through it, becomes longitudinally magnetized upon torsion.

I have now extended these experiments to the case of nickel wires. They were made essentially in the same way as the former ones. The wire, 52 centim. long and 2 millim. thick, was stretched east and west between a clamp, *a*, screwed tight, and a clamp, *b*, coaxial with *a*, capable of rotation in an up-

right, and provided with a divided circle. The wire was placed end on in front of a magnetic steel mirror swinging in a thick copper box, with the fixed clamp at a distance of about 20 centim. from it. The current of a Bunsen battery was led to this
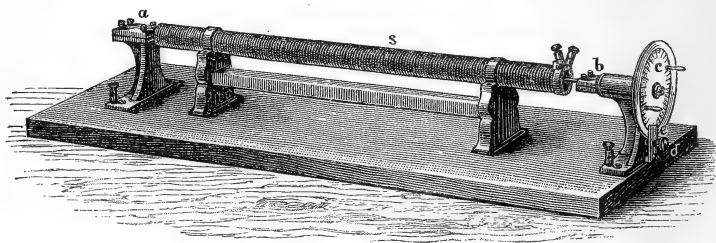
* 'Electricity and Magnetism' (2nd ed.), ii. p. 86.
† "Magnetism," *Encyclop. Metropol.* p. 270.

clamp by long wires, placed as axially as possible, which ran at first in the direction of the axis of the wire, then coming together in a wide arc, were wound round each other and carried to the poles of the battery. The deviations of the magnetic mirror were read off upon a scale at a distance of 1 metre. In order to determine the intensity of the current as well as the temporary moments of the wires, a wire ring (Plate I. fig. 1) of 7 centim. diameter was placed in front of the mirror and coaxial with it, cut through at the bottom, and capable of vertical rotation about a horizontal axle of brass, attached to it at its lowest point. The two halves of the axle were provided with copper disks dipping into mercury-troughs. A brass wire was first of all stretched in the torsion-apparatus, the current passed through it and in the proper direction through the wire ring, and this moved axially in front of the steel mirror until the latter showed no deviation, and remained without deviation when the wire was twisted. If, then, the wire ring was turned about its axis into a horizontal position, the deviation of the deflection of the mirror corresponded to the intensity of the current. Employing wires of iron or of nickel, the deflection in the vertical position of the wire ring gave the temporary moment, and in the horizontal position the same increased by a quantity proportional to the intensity of the current.

Frequently repeated experiments gave decisively the following results :—

If the current passes through an iron wire from the movable clamp to the fixed one, and if the former, as seen in the direction from the movable to the fixed clamp, is rotated in a direction opposite to the hands of a watch, the iron wire acquires a north pole at the fixed clamp, and a south pole with opposite rotation of the clamp. With reversed directions of the current opposite polarities are produced, thus confirming former results. The same takes place after stopping the current.

But if a nickel wire be employed instead of an iron wire, the results are reversed. If the current passes from the movable to the fixed clamp, and if the former be rotated in a direction opposite to that of the hands of a watch, the wire at the fixed clamp becomes a south pole, and with reversed rotation a north pole. Here also the same result is obtained after stopping the current.

Similar results were obtained also, within certain limits, upon repeated torsion in opposite directions. The deviations of the magnetic mirror far exceeded 50 or 100 scale-divisions, with torsion of $\pm 60°$ in the case of wires 2 millim. thick, so

that there could be no possible doubt of the facts. The expe-
riments may even be made with an ordinary magnetic needle
in place of the reflecting magnet. The effect is less marked
with thinner wires.

If the torsion acted only like an extension of the wires, it
would not be easy to understand why an equatorially mag-
netized wire should become longitudinally magnetized by it.
If we further assume that the fibres of the wires take up an
inclined position, but that the molecules composing them
retain their position relative to the axes of the fibres, then in
all cases, if the current flows from the movable to the fixed
clamp, and the clamp is rotated opposite to the direction of
the hands of a watch, the wire would acquire a south pole at
the fixed clamp, equally whether it consisted of iron or nickel.
The expansion of the fibres might produce under the critical
magnetization an increase of polarization in the case of iron
and a decrease with nickel, but not a reversal of the polari-
zation.

A simple displacement of the sections, without a consequent
rotation of the molecules, would communicate no longitudinal
magnetism to the wire at all, and the expansion in length
could produce no change.

Hence under all conditions torsion must produce a rotation
of the molecules of iron opposite to that which takes place in
nickel. I have already referred these rotations to the two
simultaneous phenomena just mentioned, namely, to the rela-
tive displacement of the longitudinal fibres, and that of the
sections of the twisted wires, whereby contiguous mole-
cules suffer rotation in consequence of mutual friction, and in
opposite directions in consequence of the two displacements.
In iron the friction of the longitudinal fibres is the greatest ;
in nickel that of the sections predominates.

We must evidently employ these various rotations of the
molecules, as I have endeavoured to do, to explain reciprocal
phenomena. If the molecules of a wire are placed with their
axes oblique by means of two currents passing through and
round the wire, its fibres and sections must follow their rota-
tions, as the molecules follow the displacements of the fibres
and sections when torsion takes place, and consequently here
also nickel must behave oppositely to iron. The change in the
length of the fibres can only exert a secondary quantitative
change upon the phenomenon.

Hence I believe that the phenomena I have observed cannot
be taken simply as secondary phenomena to those observed by
Mr. Joule and Sir W. Thomson.

§ 4.

If the changes of magnetic moment which take place upon torsion enable a conclusion to be drawn as to the rotation of the molecules, it appeared of interest to investigate the processes connected with the changes still further ; and first to examine a wire subjected to repeated twisting and untwisting within certain limits.

These experiments were made with the same apparatus, and by the same methods as described above :—Wires, in some cases (temporarily or permanently) longitudinally magnetized by currents led round them, in other cases (temporarily or permanently) transversely-circularly magnetized by currents led through them, were twisted and untwisted through a certain number of degrees, and their moments determined from the deflection of the suspended steel mirror. The deviations produced by the magnetizing current were compensated as before. By repeatedly raising and lowering the compensating-ring, its constancy during the experiments on temporary magnetism could be controlled.

In the following tables, in order to economize space, the data are given only for the 1st 2nd, 3rd, and nth torsion and detorsion after twisting the wire to and fro so many times that upon repetition of the experiments the same results were obtained. t denotes the angle of torsion, m_t the temporary, and m_p the permanent magnetic moment.

1. *Soft-iron wire* 2 millim. thick, *temporarily magnetized longitudinally* by a current led round it.

A.

	m_t.					m_t.			
$t.$	1.	2.	3.	$n.$	$t.$	1.	2.	3.	$n.$
$\overset{\circ}{0}...$	362·5	354·5	354·1	357·7	$\overset{\circ}{180}...$	353	357·2	358	362·5
30...	367	364	364	366·3	150...	361	369·5	370·5	374
60...	358·5	376	376	378·6	120...	376	379·5	380	384
90...	346	381	383	384·8	90...	375	378	378	381
120..	343	374	375	378·8	60...	367·5	370	370·5	372
150...	343	363·2	364	368·1	30...	360·5	361	360·5	363·6
180...	343	355	355	360	0...	354·5	354·5	355	358
210...	343	348·5	348·2	353					

B. A similar iron wire magnetized by a very weak current.

t.	1.	n.	t.	1.	n.
0° ...	116	161·8	180° ...	148	166·5
30 ...	155	169	150 ...	162·5	178
60 ...	147	184·4	120 ...	189·5	201
90 ...	140·3	197	90 ...	180·5	191
120 ...	140·7	189	60 ...	170	177
150 ...	140·2	174	30 ...	163·2	167·5
180 ...	140·5	166	0 ...	159	162·3
210 ...	140·6				

C. A similar iron wire magnetized by a very strong current.

t.	1.	n.	t.	1.	n.
0° ...	385·5	382·5	180° ...	380·5	381·4
30 ...	387·5	386	150 ...	384·4	385
60 ...	384·7	388·5	120 ...	385·8	387·5
90 ...	381	388	90 ...	386	387·8
120 ...	378·5	386	60 ...	385	385·4
150 ...	377	383	30 ...	884	384
180 ...	376·7	380·4	0 ...	383	382·4
210 ...	376·7	378			

D. An iron wire of 3 millim. diameter, magnetized in the same way.

t ...	0°	30°	58°	60°	90°	120°	150°	180°	210°
n ...	272	293	307	308	311	298	285	276	267

t ...	180°	150°	133°	120°	90°	60°	30°	0°	
0 ...	276	292	299	303	302·5	296·1	289	282	

The degrees 58 and 133 correspond to the positions of mechanical equilibrium, after removing the torsional forces, with increasing and decreasing torsion respectively.

2. *Soft-iron wire* 2 millim. thick, *permanently magnetized longitudidally* by a current led round it.

	mp.				mp.		
t.	1.	2.	n.	t.	1.	2.	n.
0°	298	68·2	42	180°	147	82·6	48
30	213	70	42·6	150	145	84	48·1
60	172	73	43·1	120	125·6	88·5	45
90	153·6	89·2	44·4	90	94	70	47
120	152·2	93	51·3	60	78	62·5	47
150	147·7	88	50·3	30	72	58	42·5
180	145·8	84	48·5	0	68·2	55·6	42
210	145	81·3	48·2				

3. *Soft-nickel wire* 2 millim. thick, *temporarily magnetized longitudinally* by a current led round it.

	$mt.$				$mt.$		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°......	63·4	88·4	89·4	180°......	79·6	80·4	83·2
30......	80·6	80·4	83·2	150......	74·4	75·4	77·2
60......	86·2	76·6	77·4	120......	79·9	81·2	82·2
90......	87·4	83·6	83·0	90......	83·6	84·4	86·2
120......	87·4	87·4	87·2	60......	86·4	87·0	88·4
150......	87·6	88·4	89·4	30......	87·4	88·4	89·2
180......	87·7	88·7	89·7	0......	88·4	88 6	89·3
210......	87·9	88·6	89·7				

4. *Soft-nickel wire* 2 millim. thick, *permanently magnetized longitudinally* by a current passed round it.

	$mp.$				$mp.$		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°......	41	−16	13	180°......	54	38·5	−17
30......	42	−12·8	11·6	150......	38	27·5	−13
60......	54	− 3·3	7·3	120......	18·2	13	− 5·6
90	60	+11·2	− 3	90......	4	0	+ 2·8
120......	62	24·2	−11	60......	− 5·3	− 9	+ 8
150......	63	34·4	−14·8	30......	−12	−15	+11·2
180......	63	40·7	−18	0......	−16	−19	+13
210......	63	43·4	−18·2				

5. *Soft-iron wire,* 2 millim. thick, *temporarily magnetized transversely* by a current passed through it.

	$mt.$				$mt.$		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°......	0	−121	−118·8	180°......	90	100	106·2
30......	48	−121	−118·4	150......	68	81	86·3
60......	83	− 88	− 92	120......	− 20·2	− 2·2	+ 2
90......	89·6	+ 36	+ 24	90......	− 92	− 87	− 84·5
120......	90·8	88·6	90·6	60......	−114	−113	−112
150......	90·5	100·3	106	30......	−119·8	−119·4	−118·2
180......	90·5	101	107·5	0......	−121	−120	−118
210......	90·5	99·5	105·2				

6. *Soft-iron wire* 2 millim. thick, *permanently magnetized transversely.*

	m_p.				m_p.		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°	1	−5·2	8·5	180°	94	52·6	30·8
30	100·5	−5	9	150	88·2	49·5	28·8
60	101	0	10·2	120	40·3	29	20
90	96·5	34	18	90	7·2	11·2	14·6
120	95	52·3	29·8	60	−1·5	4	11
150	94·1	54·5	31·8	30	−4·6	1·5	9·5
180	93·8	53	31·5	0	−5·2	0	8·2
210	93	52·5	31				

7. *Soft-nickel wire* 2 millim. thick, *temporarily magnetized transversely.*

	m_t.				m_t.		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°	0	+86·3	+86·1	180°	−69·8	−72	−75
30	−76	+70	+73·5	150	− 6·5	− 9	−19
60	−84·2	−29·8	− 6	120	+50	+52	+54
90	−85·2	−76·5	−68	90	+72	+73	+75·6
120	−85	−85·2	−84	60	+80·8	+81·6	+83
150	−85	−87	−87·8	30	+84·6	+85	+86
180	−85	−87	−88·8	0	+86·3	+86·2	+86
210	−84·6	−82·2	−87·8				

8. *Soft-nickel wire* 2 millim. thick, *permanently magnetized transversely.*

	m_p.				m_p.		
$t.$	1.	2.	$n.$	$t.$	1.	2.	$n.$
0°	+ 0	+6	+0·6	180°	−17·5	+1	+3
30	−20	+5·5	+0·6	150	− 4·5	+3	+3
60	−24	+1·5	+0·6	120	+ 3	+4	+2·5
90	−22·8	0	+1	90	+ 4·5	+3·2	+1·5
120	−22·5	0	+3	60	+ 5·2	+3	+1
150	−22·5	0	+3·2	30	+ 6	+3·2	+1
180	−22	0	+3·5	0	+ 6	+3·2	+0·6
210	−22	0	+3·2				

The magnetism of the nickel wire had diminished so much after repeated torsion that measurements with it could not be made use of.

The results of these observations for the last torsion and detorsion after attaining a permanent condition, are represented in Plate I. figs. 2–9, by curves whose abscissa represent torsions and ordinates the corresponding magnetic moments. The curves marked I., II., III. represent the results obtained with the first, second, and third torsion and detorsion of the soft-iron bar.

The curves show that after the wires have been accommodated by repeated deformation, the curves representing the magnetism are nearly symmetrical for rising and falling torsion, beginning from the minimal and maximal torsions of 0° and 210°. With the temporarily longitudinally magnetic iron wires—the moments are nearly equal for the later torsions— they rise similarly in consequence of torsion and detorsion, and then fall again to the limiting-point. The curves, however, rise more rapidly than they fall, so that the maximum of the temporary movements is obtained before the half-torsion or detorsion.

With the iron wires permanently magnetized longitudinally and temporarily magnetized transversely, the behaviour is similar, only the moments at the two limiting-points of 0° and 210° are different. Starting, however, from these points, the curves run similarly. The moments again change more rapidly at the beginning of torsion or detorsion than at the end. With nickel the moments change in the opposite direction ; but in other respects according to the same rules as with iron.

We may hence conclude that, within certain limits, the molecules, in consequence of repeated twisting to and fro, become movable ; so that they perform nearly equal motions for equal torsions from the two limiting values. With temporary longitudinal magnetization the tendency of the magnetizing-force is to place the molecules with their magnetic axes longitudinal, in consequence of which a final condition is attained in which they deviate at the limiting-points equally from their most nearly longitudinal position. *A priori*, we should expect that they would be most nearly longitudinal in the mean position, *i. e.* at half torsion and detorsion ; and there the wire would show its maximum of longitudinal magnetism. The deviation from this result shows that at the commencement of torsion or detorsion the molecules are more quickly influenced by the acting forces.

It is also to be observed that the permanent torsions into which the bar, left to itself, slowly passes from the temporary torsions 0° and 210° do not at all agree with the torsions at which it shows the maximum of temporary magnetism ; these

last lie further away from the extreme torsions than the permanent torsions do.

If we consider the changes in temporary magnetism in the first torsion, we observe in every case an increase and then a decrease, evidently because the molecular magnets, directed more or less axially by the magnetizing-force, obey at first that force still further in consequence of vibration, and are then carried from their axial position into more oblique positions. This is confirmed by the fact that with very strong temporary magnetism the first rise through vibration is less than with less powerful magnetization, since in the first case the molecules are already more nearly in the axial position.

With permanent longitudinal magnetization the directing force of the external magnetizing-power is absent; the molecules, disregarding mutual action, are affected only by the displacements produced by torsion, which consequently manifest themselves more distinctly than in the previous experiments. The moment at the limiting-points is different, and thus also the position of the molecules. But, again, the most rapid rotation of the molecules at the beginning of each deformation is seen, within the given limits.

A similar behaviour is observed in the case of temporary transverse magnetization, in which the magnetic directing-force which acts upon the molecules is one-sided; and so the moments at the limiting-points cannot be equal. Nevertheless, the rotations of the molecules with rising and falling torsion here also preserve the above-mentioned character.

§ 5.

In exact analogy with the mechanical and magnetic behaviour described in § 4, it may be shown that if a wire which has been subjected to a temporary deformation (as a special case, this deformation may be zero), then suffers another deformation, the molecules again retain the position corresponding to the first deformation up to a certain degree.

In order to make these experiments, a wire was temporarily twisted by forces 0, a, b, gradually increasing, and its temporary torsion measured, as also with decreasing forces b, a, 0. For this purpose the same apparatus was used, as in my former experiments. A flat weight $\gamma \gamma_1$ provided with a hook was attached to the thread $o q$ which acts upon the torsion-circle; to this a second weight $\beta \beta$, with a hole through the centre, was attached by lateral threads $\alpha \beta \gamma$, $\alpha_1 \beta_1 \gamma_1$. These threads were connected to a horizontal wire $\delta \epsilon$, which was soldered to the frame of a pulley η suspended by the thread $\zeta \theta$. The end θ

of this thread was attached to a fixed hook, the end $\zeta\,\theta$ passed over a fixed pulley to the windlass z. If by turning ζ the pulley η was gradually lowered, the weight $\gamma\,\gamma_1$ at first acted upon the torsion-circle until the weight $\beta\,\beta_1$ reached the other, when both together acted in twisting the wire. The horizontal wires soldered to the weights rested against fixed vertical wires, and so prevented the turning of the weights with the threads carrying them. By winding up the pulley η, the torsion corresponding to the weights $\beta\,\beta_1+\gamma\,\gamma_1$ and that corresponding to the weight $\gamma\,\gamma_1$ alone could be determined. The weights were always raised and lowered very slowly, so that they produced their effect without any jar. The reading each time was not taken until, after putting on the weights, the temporary torsion did not perceptibly change any more. The weight $\gamma\,\gamma_1$ (1) weighed 61·8 gr., $\beta\,\beta_1+\gamma\,\gamma_1$ (1) + (2) 125·9 gr.

As before, the torsions were read off by means of a telescope in a mirror, which was attached to the lower clamp of the wire subjected to torsion, and in which the image of a semicircular scale, placed round the wire as axis with a radius of 1 metre, was reflected. The following tables contain some of the results obtained. The first column gives under G the weights used to produce the torsion, the following columns give the corresponding torsions successively obtained. The experiments were continued until the same torsion was obtained upon renewed action of the weights.

I. Brass wire, 2 millim. thick and 51·8 millim. long, stretched before loading by a weight of 8350 gr., and annealed under a load of 8000 gr.

G.	I.	II.	III.	IV.	V.	V1.
0	0	223·5	242·5	255·5	264·2	267·3
(1)	684·0	904·5	924·5	937·0	943·0	947·0
(1)+(2)	1691·7	1705·0	1706·7	1713·5	1714·0	1714·5
(1)	973·0	989·5	693·5	990·5	1000·5	1001·5

G	VII.	VIII.	IX.	X.	XI.
0	269·5	273·0	273·0	275·0	281·0
(1)	949	950·5	952·0	953·5	957·2
(1)+(2)	1714·5	1714·5	1718·5	1719 5	1721·5
(1)	1002·5	1002·5	1005·0	1004·3	1000·5

After ten repetitions of the increase and decrease the deflections obtained were :—

G.	XXI.	XXII.
0	......	287·5
(1)	......	965·7
(1)+(2)	1717·8	......
(1)	1000·5	......

A similar wire annealed while loaded with the stretching weights gave the following values:—

Torsion.

G.	I.	II.	III.	IV.	V.
0	0	232·5	250·2	262·0	266·5
(1)	701·3	928·5	947·5	962·3	......
(1)+(2)	1711·5	1723·7	1736·5	1733·7	......
(1)	982·5	990·5	999·5	999·0	......

After action of the weights, repeated ten, twenty, and thirty times, the deflections were:—

G.	XXV.	XXXV.	XXXVI.
0	274·0	276·0	276·0
(1)	976·5	975·0	976·0
(1)+(2)	1731·5	1729·5	1729·5
(1)	996·7	999·5	997·0
0	274·5	276·0	276·0

Next the wire was caused to vibrate strongly so as to emit a musical note by striking it with a flat board whilst loaded with the weight (1) during the increase as well as during the decrease of the weights, and also whilst loaded with the weights (1) + (2). The values thus obtained are given within brackets, against the values obtained before the vibration:—

G.	XXXVII.	XXXVIII.	XXXIX.	XL.
0	276·0	292·5	391·5	419·5
(1)	977·0 (984·5)	988·7	1092 (1095·5)	1121·5
(1)+(2)	1738	1746·0 (1861·5)	1853·5(1890·5*)	1880·5
(1)	1000·5 (1007·5)	1120·5	1150·5	1145·5
0	292·5	391·5	419·5	420·5

* Made to vibrate powerfully and for a long time.

It follows from these experiments that the temporary torsion of the wires, produced by the smaller weight after the action of the greater weight, with a decreasing load, is in every case greater than that produced by the action of the same weight with increasing load, and before the action of the greater weight. The behaviour is observed not only during the accommodation of the wire, but also after it has attained a permanent condition, when a repetition of the cycle of experiments gives the same results. It also is not altered by strong vibration of the wire.

We may hence assume that, in the action of any torsional forces whatever (which may also be zero), the molecules are displaced and rotated in accordance with the deformation produced by these forces, and afterwards, upon the action of other forces, pass from their first positions of equilibrium into others, which, however, are conditioned by the first. If, then, the molecules are greatly displaced and rotated by a more powerful torsion, they still partially retain this displacement and rotation upon the action of a feebler force; whilst, conversely, upon changing from a feebler to a stronger torsional force, the latter is unable to produce so great a displacement and rotation of the molecules which lie nearer their neutral position.

A similar behaviour in the case of wires is shown by an experiment of H. Tomlinson's[*], in which, however, only small differences were observed between loading and unloading.

§ 6.

The behaviour of iron bars in temporary magnetization, by increasing and decreasing forces, is exactly similar to the mechanical behaviour here described. Messrs. Righi[†], Fromme[‡], and Warburg[§] have shown that if an iron bar is successively magnetized by forces a, b, c, b, a, of which $a < b < c$, the temporary moment M'_b, corresponding to the force b with decreasing magnetization, is greater than the temporary moment M_b, corresponding to the same force with increasing magnetization. Warburg has ingeniously deduced from this the behaviour of a magnetic needle vibrating above an iron plate.

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[*] Phil. Trans. Roy. Soc. Lond. 1883, Part I. p. 10 (Exp. V.). Tomlinson, in his experiments, put on the weights by hand without observing irregularities; it seems to me that in the method now described, which was used by me in 1858, better security against accidents is obtained.

[†] *Mem. di Bologna*, 20 Mai, 1880; *Beibl.* v. p. 62 (1884).

[‡] Wied. *Ann.* iv. p. 102 (1878), xiii. p. 318 (1881).

[§] Wied. *Ann.* xiii. p. 141 (1881).

In order to investigate whether this phenomenon occurs only with the first increasing and decreasing magnetizations of the bars, or whether it occurs also after they have attained a permanent condition after repeated action of the magnetizing forces, the following experiments were made, in which freshly heated iron bars were magnetized by currents passed round them in such a way that their intensity could be gradually increased up to a certain magnitude and then gradually reduced to zero, so avoiding the influence of all disturbing induction-currents.

For this purpose a special regulating Bunsen's element was employed (fig. 11). It consisted of a porcelain cylinder 36 centim. high and 10 centim. wide, on the bottom of which was cemented a second concentric porcelain cylinder closed at the top, 25 centim. high and 5 centim. wide. This supported a porous cell 10 centim. high and 5 centim. wide, in which was contained a Bunsen carbon-cylinder provided with a connecting wire, and was filled with nitric acid. It was surrounded by a cylinder of amalgamated zinc 10 centim. high and 9 centim. wide, which was suspended by means of strips of zinc soldered to it from the edges of the outer porcelain cylinder, which contained dilute sulphuric acid. A glass tube 40 centim. long closely surrounded the porous cell and its support, and could be raised or lowered by means of a cord attached to it, which passed over a pulley to a roller and handle. A brass band stretched by a weight pressed against the roller and held it firmly in any position. By this arrangement the current could be increased from nearly zero up to a considerable strength. Of course the same arrangement might be employed as a Daniell cell*.

The current from this cell was passed through a copper-wire spiral 25 centim. long, 2·7 centim. internal diameter, and 7 centim. external diameter, lying horizontally magnetic east and west, 50 centim. distant from the steel mirror of the reflecting-galvanometer. The deviation thus produced was compensated by means of the vertical copper ring placed in front of the mirror. A freshly heated bar of soft cast steel, 24 centim. long and 1 centim. thick, was fixed in the spiral, and its momentum determined from the deflection of the mirror. The corresponding intensity of the current was determined as before, by turning down the ring. In order, further, to be able to adjust the current during the temporary magnetization of the bar to any desired strength, the ends of the spiral were connected, in particular cases, with the quadrants

* Meanwhile a similar arrangement has been described by Stebbins, *Centralbz. f. Opt. u. Mech.* iv. p. 119 (1883); *Beibl.* vii. p. 474 (1883).

of a Mascart's electrometer, the needle of which was charged from a battery of 100 copper-water-zinc elements. Its deflection was read by means of a telescope and scale distant 1 metre from the mirror attached to the needle.

In the following Tables E denotes the deflection of the needle of the electrometer, M the temporary moment of the magnet, I the intensity of the current measured by lowering the ring.

In the first series of experiments the iron bar was repeatedly magnetized by currents which in each case rose to a definite maximum intensity determined by the electrometer, and were then reduced to zero. The permanent moment was determined in each case after complete cessation of the current ($E=0$). The results obtained were as follows :—

| (1) | E | ...... | 5 | 13 | 13 | 17 | 23 | 17 | 12·8 | 9·5 | 6 | 0 |
| | M | ...... | 55·5 | — | 61 | 157 | 379 | 323 | 278 | 231 | 177 | 99 |

| (2) | E | ...... | 5·5 | 9 | 13·2 | 18 | 23·5 | 17·7 | 12·7 | 9 | 5·8 | 0 |
| | M | ...... | 146 | 184·6 | 230·2 | 291·3 | 381 | 323 | 269 | 228 | 184 | 109·5 |

| (3) | E | ...... | 5·8 | 9 | 13 | 18 | 23·2 | 16·7 | 13 | 9 | 5·8 | 0 |
| | M | ...... | 155 | 190·6 | 234·5 | 301·2 | 381 | 309 | 270·5 | 228·2 | 185 | 113 |

| (4) | E | ...... | 5·8 | 8·9 | 13 | 18 | 25 | 17·5 | 13 | 9 | 5·8 | 0 |
| | M | ...... | 159·2 | 190 | 236 | 296 | 394·2 | 322·2 | 272 | 229·8 | 187·5 | 116·2 |

and after *ten* similar magnetizations,

| (15) | E | ...... | 5·8 | 9 | 13 | 17 | 23 | 16·5 | 13 | 9 | 5·8 | 0 |
| | M | ...... | 173 | 206 | 249·3 | 303 | 380·5 | 319 | 281·5 | 237·6 | 194·2 | 126·2 |

In another series of experiments a fresh iron bar was repeatedly magnetized to the same temporary maximum moment M_t; then the intensity of the current was decreased to zero, and the permanent moment M read off after complete cessation of the current.

Thus :—

	1.	2.	3.	4.	5.	10.
M_t	320	320	320	320	320	320
m	90	94	96	97	97·5	98·5

and the temporary moment M after the eleventh magnetization :—

I	14	20·7	27	34	42·5	52·2	62	71
M......	137	157·5	176	200	226	258	294	317
I	60·8	45·5	36·7	27	21·3	15·8	14	
M......	292·2	254·5	225·6	199	176	155·2	150	

It follows from these observations that even after an iron bar, by repeated temporary magnetization, either to the same maximum of temporary magnetism, or by currents which always attain the same maximum of intensity, has attained a permanent condition, the temporary moment corresponding

to a given magnetizing force is always greater with decreasing forces than with increasing forces.

Here, also, there prevails then a complete analogy to the mechanical behaviour under torsion produced by increasing and decreasing torsional forces.

§ 7.

The gradual accommodation of the molecules upon repeated torsion within certain limits, described in the foregoing pages, as recognized by magnetic behaviour, in which the deflections with the same torsion increase at first, agrees with the result of my former experiments [*], that the permanent torsions of a wire, upon repeated temporary twistings of the same up to a certain limit, and also in the same way upon repeated action of the same torsional force, increases gradually up to a maximum.   This behaviour shows clearly that the molecules in the deformed wire are only carried gradually from the position of equilibrium corresponding to the zero into that which corresponds to the deformation.   The same holds good of the action of magnetic forces tending to rotate the molecules. Here, also, as the second Table, § 6, shows, the permanent moment increases gradually up to a certain maximum, upon repeated temporary magnetization up to a certain limit.

In the same way, as the above experiments show, an iron bar only attains, after the repeated action of a given magnetizing force, the maximum magnetic moment corresponding thereto.

This last-mentioned behaviour has already been observed by Quetelet [†] in repeatedly rubbing steel needles with a magnet, and by Hermann and Scholz upon repeated contact with the pole of a steel magnet, and similarly by Bouty [†] and Fromme [†] upon the repeated introduction of iron bars into magnetizing spirals traversed by a constant current [‡].   In this last method, the separate portions of the bars are successively exposed to the magnetizing forces of different intensity at the different points of the spiral, and thus the position of the molecules of the bars already taken up at one point may assist the adjustment of the molecules at other points in the subsequent introduction into the spiral, or rubbing, and thus explain the increase of the permanent moments.

    [*] G. Wiedemann, Wied. *Ann.* vi. p. 495 (1879).

    [†] On the literature of the subject, see G. Wiedemann, *Elect.* iii. p. 442, &c.

    [‡] I have formerly obtained a series of results on the magnetization and demagnetization of iron and steel bars by the same method.   Experiments by the method now employed with constant distribution of the magnetizing forces give (qualitatively) the same results.

That this is also the case with an unchanged distribution of the magnetizing forces is seen from the above experiments.

§ 8.

In close connection with these phenomena is the thermo-electric behaviour of stretched wires towards the same un-stretched, studied by Cohn[*], which is certainly one of the most sensitive modes of detecting changes in structure. Here, also, the thermoelectric forces which correspond to equal tensional forces are different with increasing and with decreasing tension. There could be no doubt that the in-vestigation of the thermoelectric behaviour of wires twisted to and fro would give similar results, and this is confirmed by experiment; as also in the investigation of the same behaviour with iron and steel bars, which are magnetized by increasing and decreasing magnetizing forces. In so far as the torsion or detorsion affects the electric conductivity of the wires, it must show analogy to the magnetic behaviour. This much may, however, safely be concluded from these investigations, that if the molecules of the bodies are dis-placed or rotated from their former positions of equilibrium, before as well as after the molecules have accommodated themselves by the repeated action of the force, they will always, to some extent, also retain this displacement or rotation upon gradual alteration of the force.

This is seen in various ways. Thus, for example, if the force influencing the body is reduced to zero, a permanent dis-placement and rotation remains; again, in order to reduce a permanent torsion or magnetization to zero, a smaller force is necessary than is required for the production of the same. Further, a body which has accommodated itself by the re-peated action of increasing and decreasing forces, and is then again repeatedly exposed to the action of the same forces, shows each time, more or less, the position of the molecules corresponding to the greater or smaller force preceding that allowed to act last.

Since it is not possible, in the present condition of our knowledge, to express these phenomena of elasticity in the widest sense of the word by mathematical laws under appro-priate theories, further experimental researches on this subject are required, in which the time during which the forces act, and the time which elapses after they cease to act, must be taken into account. A thorough study of the magnetic pro-perties of bodies, from which we can infer, up to a certain

[*] Wied. *Ann.* vi. p. 385 (1879).

point, the rotations of the molecules, seems to be especially suited to this purpose.

§ 9.

The behaviour of bodies under changes of temperature is quite different from their magnetic and mechanical behaviour.

If a body, whether deformed or magnetized, has been accommodated by repeated heating and cooling, so that the molecules have assumed their final mean position of equilibrium for each single temperature degree, the same mechanical or magnetic condition corresponds to the same temperature, whether with rising or falling temperature. Thus a thermometer, after repeated changes of temperature, gives the same indications at the same temperature, whether with rising or falling temperature, when once the extreme indications have become constant. So also a thermo-element (say of copper and german silver) shows, after accommodation, always the same electromotive force, and a magnet always the same permanent moment at a given temperature, as is shown by direct experiment. The difference is just this, that in mechanical deformation and magnetization the molecules have been carried over into new more or less stable positions of mechanical equilibrium, by means of mechanical displacements and rotations, out of which they may be directly displaced by fresh mechanical influences ; whereas by heating we change only the amplitude of the vibrations of the molecules in all directions above the same position of equilibrium.

VIII. *On the Seat of the Electromotive Forces in the Voltaic Cell.   By* Prof. W. OSTWALD.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN accordance with a request made in the annexed portion of a letter just received, may I ask you to insert it in your next issue.

Your obedient servant,
OLIVER LODGE.

Riga, June 2, 1886.

SIR,—After reading your memoir on the seat of E.M.F. in the pile, published in last year's Philosophical Magazine*, allow me to express my full agreement with your views. But I further believe that it will interest you to know that there is a method of directly measuring differences of potential, whether between two liquids or between a liquid and a metal.

* Phil. Mag. October 1885, p. 372.

The method rests upon a remark of Helmholtz*, that a quantity of mercury, if insulated, and arranged to form drops quickly beneath the surface of a liquid, rapidly acquires the potential of the liquid. Let T (fig. 1) be an insulated dropping-funnel full of mercury, which drops inside the liquid F through a fine point, and let M be a metal plunged into the same liquid; the electrometer E will indicate the true difference of potential between M and F. I have experimentally convinced myself that, by careful regulation of the flow, one can get the potential of the mercury to correspond with that of the liquid within about ·01 volt.

Fig. 1.
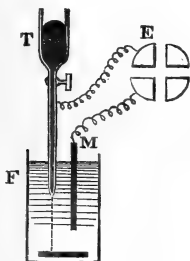
Fig. 2.
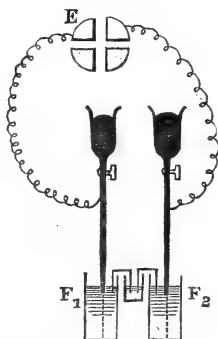
In order to measure the potential-difference of two liquids, one uses two funnels (fig. 2), and connects each of the two liquids F_1 and F_2 with a third vessel containing one of them by means of a capillary siphon, so as to prevent any mixture. I have to thank my friend Dr. Arrhenius for the suggestion of the two funnels. I have found by this method that pretty big potential-differences frequently occur between liquids, going up to ·8 or ·9 volt with dilute solutions. The E.M.F. of a Daniell cell appears to be about ·8 volt between zinc and zinc-sulphate, ·3 volt between copper and copper-sulphate; while between zinc-sulphate and copper-sulphate the difference seems practically zero. Nevertheless, since copper salts have shown some hitherto unexplained phenomena, I do not yet quite regard this result as indubitable.

I am at present so immersed in literary work that I cannot continue my investigations for several months; but I should be grateful if you will translate some of this and send it to the Editors of the Philosophical Magazine.

<div align="center">

Believe me, &c.,

Dr. WILH. OSTWALD,

Prof. Polytechnicum, Riga, Russia.

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* *Monatsber. Berl. Ak.* November 1881.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxi. p. 514.]

May 12, 1886.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Maxilla of *Iguanodon.*" By J. W. Hulke, Esq., F.R.S., F.G.S.

2. "Notes on the Distribution of the Ostracoda of the Carboniferous Formations of the British Isles." By Prof. T. Rupert Jones, F.R.S., F.G.S., and J. W. Kirkby, Esq.

3. "Note on some Vertebrata of the Red Crag." By R. Lydekker, Esq., F.G.S.

4. "The Pleistocene Succession in the Trent Basin." By R. M. Deeley, Esq., F.G.S.

The author, after referring to previous publications on the subject, proceeded to notice the leading characters of the greatly developed Pleistocene deposits in the area drained by the Trent river and its tributaries. He proposed to classify the beds in question under three divisions, comprising the following stages. The beds of the lowest division were distinguished from those of the middle and upper by the absence of Cretaceous rock-débris.

OLDER PLEISTOCENE.
Early Pennine Boulder-clay.
Quartzose Sand.
Middle Pennine Boulder-clay.

MIDDLE PLEISTOCENE.
Melton Sand.
Great Chalky Boulder-clay.
Chalky Sand and Gravel.

NEWER PLEISTOCENE.
Interglacial River-alluvium.
Later Pennine Boulder-clay.

Each of the separate stages was then described separately, with details of exposures and sections throughout the area.

The Early and Middle Pennine Boulder-clays, which closely resembled each other, were regarded as composed of materials derived almost entirely from the Derbyshire mountains, but with a slight admixture, to the westward, of erratics derived from Scotland and Cumberland. The latter were probably brought from those localities by an ice-stream, the main materials of the deposits having been transported from the Pennine chain by glaciers, and deposited in the partially submerged valley of the Trent. The intermediate quartzose sand was deposited in the sea during an intercalated warmer age of considerable submergence.

The Middle Pleistocene deposits, distinguished from the earlier by containing large quantities of chalk and flints derived from the north-

east, were apparently formed at a time when the level of the Trent-valley area was lower than that of the Cretaceous tracts in Lincoln-shire and Yorkshire. The Great Chalky Boulder-clay was chiefly a ground-moraine formed beneath an ice-sheet on land, but in places presented signs of aqueous origin. The Melton sand, below, in which Cretaceous detritus first appeared in abundance, consisted of stratified sands with occasional beds of gravel or loam, and indicated a less extreme temperature. In West Staffordshire the gravels and sands probably represented the entire Middle Pleistocene epoch, no great Chalky Boulder-clay being found, and in this area frag-ments of marine mollusca were of frequent occurrence. The Chalky Gravel was also a marine deposit, and, like the Melton Sand, was probably formed when the temperature was rather milder than it was during the deposition of the Great Chalky Boulder-clay.

In the Newer Pleistocene epoch re-elevation of the Trent valley and of the Pennine chain appeared to have again produced a change in the direction from which the materials of the deposits were derived. The Interglacial Alluvium was of freshwater origin, but the admixture of Scotch and Cumbrian detritus with that derived from the Pennine range indicated that glaciers from the north again reached the Trent area. A colder age succeeded, during which the Later Pennine Boulder-clay was formed, partly of local materials, partly of erratics from the Pennine range, mixed with a few from Cumberland and even from Wales. This deposit was almost entirely unstratified, and consisted largely of moraine detritus, the ice-sheets having disturbed and rearranged the earlier deposits and mixed them with rock-detritus from the neighbourhood. To this later ice-sheet was attributed the contortion so frequently observed in the older and middle Pleistocene deposits. Reasons were given for the opinion that such contortions were due to ice-and not to soil-cap motions or their later agencies.

5. " On the Existence of a Submarine Triassic Outlier in the English Channel off the Lizard." By R. N. Worth, Esq., F.G.S.

Attention was called to the frequent occurrence of sandstone fragments in a certain part of the English Channel, brought up by the fishermen's "long lines." The evidence favours the idea that these rocks are *in situ.*

A list of the specimens found, with bearings and distances, was given ; they consist of red, and sometimes greyish sandstones, mostly soft, also marls, " potato stone," and nodules of Triassic trap. The affinities are with the Keuper of Devon. The position deduced from the observations is about 10 miles S.E. of the Lizard, and beyond the 30-fathom line. This submarine outlier is larger than any outlier on the mainland of Devon or Cornwall, and carries the English Trias nearly 50 miles further to the S.W.

April 21.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read :—

1. " Further proofs of the Pre-Cambrian age of certain Granitoid, Felsitic, and other Rocks in North-western Pembrokeshire." By Henry Hicks, M.D., F.R.S., F.G.S.

In this paper the author gave the results obtained by him during a recent visit to N.W. Pembrokeshire. He stated that he had further examined some of the sections referred to in his previous papers, as well as others not therein mentioned, and that he had obtained many additional facts confirmatory of the views expressed by him in those papers. The lower Cambrian conglomerates and grits, he said, contained pebbles of nearly all the rocks in that area which he had claimed as of pre-Cambrian age; and the fragments of the granitoid rocks, the felsitic rocks, the hälleflintas, and of the various rocks of the Pebidian series which he had found, showed unmistakably that those rocks had assumed, in all important particulars, their peculiar conditions before the fragments were broken off.

Moreover, he stated that there was abundant evidence to show that the very newest of the pre-Cambrian rocks of the area had been greatly crushed, cleaved, and porcellanized, before any of the Cambrian sediments were deposited; hence he maintained that there was in the area a most marked unconformity at the base of the Cambrian. At Chanter's Seat, near St. David's, he found that the lower Cambrian grits and conglomerates were, in parts, almost wholly made up of fragments of characteristic varieties of the Granitoid rocks which form the Dimetian ridge near by.

The so-called granite of Brawdy, Hayscastle, and Brimaston, he said, there was good evidence to show, was probably of the age of the Granitoid rocks of St. David's. The mass of so-called granite near Newgale, he stated, was composed of rhyolites and breccias, undoubtedly of pre-Cambrian age.

The Roch Castle and Trefgarn rocks, he stated, could not possibly be intrusive in Cambrian and Silurian strata, but belonged to a series of pre-Cambrian rocks. He referred to the important evidence bearing on the age of these rocks given in a paper communicated to the Society, since his last paper was read, by Messrs. Marr & Roberts. These authors showed that in a quarry near Trefgarn Bridge a Cambrian conglomerate, overlain by *Olenus*-shales, is to be seen resting on the eroded edges of the Trefgarn series. The author examined this section lately, and obtained from the Conglomerate some very large pebbles of the characteristic rocks called hälleflintas, and of the ash-bands, both of which are found *in situ* in the quarry. He therefore maintained that there was the most ample evidence to show that there was a great group of pre-Cambrian rocks exposed in N.-W. Pembrokeshire, and hence that he had proved conclusively that Dr. Geikie's views in regard to these rocks, as given in his paper and more recently in his text-book, are entirely erroneous.

2. "On some Rock-specimens collected by Dr. Hicks in North-western Pembrokeshire." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

The author stated that he had examined microscopically a series of specimens collected by Dr. Hicks, and compared them with those described by Mr. T. Davies, in vol. xl. of the Quarterly Journal, and with some in his own collection. He agreed with Mr. Davies's conclusions in all important matters.

The Chanter's-Seat conglomerate contained many grains of quartz and felspar, curiously like those minerals in the so-called Dimetian, together with numerous small rolled fragments, about a quarter of an inch in diameter, exactly resembling the finer-grained varieties of that rock, besides bits of felsite, similar to some which occur in the St. David's district, quartzite, a quartz-schist, and an argillite.

The rocks *in situ* in the Trefgarn quarry were indurated trachytic ashes, together with the curious flinty rock which was the most typical of the so-called hälleflintas. One of the pebbles from the overlying conglomerate perfectly corresponded with the last-named rock, others appeared to be most probably from an altered trachytic ash, differing only varietally from those *in situ*.

After prolonged examination of this " halleflinta " of Trefgarn and the similar rocks from Roch, he was of opinion that while it was possible that some specimens might be altered ashes, most of them were originally rhyolites or obsidians, devitrified, and then silicified by the passage of water which had contained silica in solution. The Trefgarn group obviously could not be intrusive in the lower Cambrian, and it was extremely improbable that the Roch Castle series was newer than the basement conglomerate of that district.

The Brawdy granitoid rock might be a granite, but at any rate it presented considerable resemblance to the " Dimetian."

It was therefore evident that the Cambrian conglomerate of St. David's was formed from a very varied series of rocks, some of them much older than it, and that the Dimetian could not be intrusive in it. Moreover, even if the Dimetian should be proved ultimately to be a granite, and the core of a volcano which had emitted the rhyolites, sufficient time must have elapsed after its consolidation and prior to the making of the conglomerate to remove, by denudation, a great mass of overlying rock. Hence, whatever its nature, it was pre-Cambrian.

3. " On the Glaciation of South Lancashire, Cheshire, and the Welsh Border." By Aubrey Strahan, Esq., M.A., F.G.S., H.M. Geological Survey. By permission of the Director General.

Part I. *South Lancashire and Cheshire.*

The average direction of the large number of glacial striæ which have been observed in the neighbourhood of Liverpool is N. 28° W. Further up the Mersey there is a slight deflection towards the east. Two instances only occur of striæ having a totally different direction, namely E.N.E. The striæ themselves seldom furnish any evidence as to the direction in which the ice travelled, but the edges of the strata have in many cases been bent back from the north-west. The materials of which the drift is composed, both matrix and included boulders, have also travelled from the north-west. The sands and gravels also are arranged in long banks, trailing away from the south-west sides of the rock-hills, in such a way as to show that they are distributed by currents from the north-west. The striæ are found in connexion with the Boulder-clays, in which the actual presence of ice is abundantly proved. Presumably the

same agent that distributed the Boulder-clay, also striated the rock-surface and moved from the north-west.

Part II. *The Welsh Border.*

The striations within the Welsh Border also show a general parallelism, but in a direction E.N.E., the few exceptions that occur being close to the Border. The direction in which the drift has been transported shows a corresponding change ; though analogous in arrangement to the Lancashire drift, it has all travelled from W.S.W. to E.N.E. The boundary between the northern drift of the English side, and the western drift of the Welsh runs approximately along the coast, bending inland here and there, and cutting inland across parts of South Flintshire and Denbighshire. The transportation of the Welsh drift has taken place across the lines of the principal hill-ranges and valleys. Occasionally the two drifts shade one into the other, or are mixed together ; but as a general rule the far-travelled northern drift overlies the more local deposit, and is easily distinguishable by its different materials and by its being comparatively stoneless.

It may be concluded that :—

1. The striæ on the English and Welsh sides respectively, while showing variations among themselves, by a marked preponderance in one quarter of the compass, indicate a direction of principal glaciation, this direction being on the English side from about N.N.W., and on the Welsh from about E.S.E.

2. The direction of glaciation in both districts agrees very closely with that of the transportation of the drift, but is only locally influenced by the form of the ground.

3. The striæ are by no means universal, but are found almost exclusively in connexion with those beds in the drift which contain evidence of the actual presence of ice.

Part III. *Origin of the Striæ.*

The striæ are not such as can have been produced by valley-glaciers ; they go across and not down the valleys, nor are there any moraines. The question resolves itself into (1) the hypothesis of two ice-sheets moving in different directions in the two areas ; (2) that of floating ice. The first is opposed by the facts that the rock-surface is not *moutonnée* on a large scale, and that the striæ and terminal curvature are far from universal ; that the drifts associated with the striæ are marine deposits ; that striæ having different directions are found on the same slab. The well-known occurrence of gravel-ore in the drift at the outcrop of a vein is also against this hypothesis. The marine origin of the drifts is indicated by their well-marked stratification as a whole, by the alternations of well-washed sands and gravels with the Boulder-clays, and by the occurrence through all the beds of marine shells. A lower or basement-clay is seen in places under this marine drift, but it is always the latter with which the striæ are associated. The great development of undoubted marine beds and comparative rarity of *moutonnée* surfaces constitute the principal differences between this region and

the north, where the existence of an ice-sheet has been strongly advocated. Anglesey is considered by Sir A. Ramsay to have received its configuration by the action of an ice-sheet from the north ; but its physical features appear to be due rather to its geological structure, and to have existed in more or less their present form in pre-glacial times.

The arrangement of drifts in this district presents an analogy with that of the Norfolk drifts, and probably results from a similar sequence of events.

The marine drifts, from their great variability, seem to have been distributed, and the striations produced by floating ice, driven by tidal or oceanic currents, during the time of submergence. During this time Snowdon and the surrounding hills must have stood well above water, forming an island-group, and by such a group the prevailing currents from the north would be deflected to the southwest over Anglesey on the one side, and to the south-east over the plains of Cheshire and Shropshire on the other, while within the limits of the group a local circulation might be maintained.

June 9.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read :—

1. "On the Volcanic Rocks of North-eastern Fife." By James Durham, Esq., F.G.S., with an Appendix by the President.

After describing the general distribution of the volcanic rocks of Old Red-Sandstone and Carboniferous age in the counties of Forfar and Fife, the author called attention to a fine section exhibited where the Ochil Hills terminate along the southern shore of the Firth of Tay. In immediate proximity to the Tay Bridge, a series of the later volcanic rocks, consisting of felstones, breccias, and ashy sandstones, are found let down by faults in the midst of the older porphyrites (altered andesites) which cover so large an area in the district. The breccias contain enormous numbers of blocks of a red dacite (quartz-andesite), and enclosed in this rock angular fragments of a glassy rock, resembling a " pitchstone-porphyry," are found, everywhere, however, more or less converted into a white decomposition-product. The youngest igneous rocks of the district are the bosses and dykes of melaphyre (altered basalt and dolerite) which have been often so far removed by weathering as to leave open fissures.

In the Appendix three very interesting rocks were described in detail. The rock of the Northfield Quarry, which is shown to be an augite-andesite, has a large quantity of a glassy base with felted microlites, and contains large porphyritic crystals of a colourless augite. The rock of the Causewayhead Quarries is described as an enstatite-andesite ; it has but little glassy base, being made up of lath-shaped felspar crystals (andesine), with prismatic crystals and grains of a slightly ferriferous enstatite ; there are no porphyritic crystals, but the enstatite individuals are sometimes curiously aggregated. The red porphyritic rock from the breccias near the Tay Bridge was shown to be a mica-dacite, and the glassy

rock associated with it to be the same material with a vitreous in place of a stony base. This· glassy base exhibits very beautiful fluidal and perlitic structures. The crystals of first consolidation in this rock are oligoclase and biotite, often showing marks of injury in transport ; those of the second consolidation appear to be orthoclase. In conclusion, the successive stages by which the andesitic rocks of the area were altered, so as to assume the characters distinctive of porphyrites, were fully discussed, as well as the change of the glassy rock into its white decomposition-product.

2. " On some Eruptive Rocks from the neighbourhood of St. Minver, Cornwall." By Frank Rutley, Esq., F.G.S.

The rocks described in this paper were derived from Cant Hill, opposite Padstow, and from a small quarry about half a mile from Cant Hill, near Carlion. At the former locality the volcanic rocks are much decomposed, but from their microscopic characters they may be regarded as altered glassy lavas of a more or less basic type. No unaltered pyroxene, amphibole, or olivine is to be detected in the specimens described, but there is a considerable amount of secondary matter which may include kaolin, serpentine, chlorite, palagonitic substances, &c. There is evidence of fluxion-structure in some of the sections ; others are vesicular, and the vesicles are usually filled with siliceous or serpentinous matter. The relation of these lavas to the underlying Devonian slates was not ascertained. The rock occurring near Carlion contains numerous porphyritic crystals of augite in which the crystallization is interrupted by the co-development of small felspar crystals, which appear, as a rule, to have been converted into felsitic matter. Ilmenite is also present in patches which indicate a similar interrupted crystallization to that shown by the augite. The rock has the mineral constitution of an augite-andesite, but since it is a holocrystalline rock, exception would be taken by many petrologists to the employment of the term andesite. The lavas of Cant Hill were also probably of an andesitic character, so that, so far as original mineral constitution is concerned. there is some apparent justification for the mapping of both of these rocks as " greenstone " by the Geological Survey.

3. " The Bagshot Beds of the London Basin." By H. W. Monckton, Esq., F.G.S., and R. S. Herries, Esq., B.A., F.G.S.

The authors stated that their object was to describe more fully the Lower Bagshot beds, and to disprove the view lately advanced by Mr. Irving that, in certain places, the Upper Bagshots overlap the Lower and rest directly on the London Clay. They described or referred to a number of sections all round the main mass, beginning at St. Ann's Hill, Chertsey, where they considered that the mass of pebbles and associated greensands must be referred to the Middle Bagshot. The outliers near Bracknell and Wokingham were shown to consist of Lower and not Middle Bagshot, which does not appear in the valley north of Wellington College.

The Aldershot district was explained, and it was shown that the beds there resting on the London Clay were Lower and not Middle

Bagshot, and the occurrence of fossils in the Upper Bagshot of that district was recorded.

The conclusions that the authors came to were, that a well-marked pebble-bed was almost always present, marking the division between the Upper and Middle Bagshots, but that there were other pebble-beds of a less persistent character occurring both in the Middle and Lower Bagshot; that the Lower Bagshots generally consist of false-bedded sands with clay laminæ and no fossils except wood, whereas the Upper Bagshots are rarely false-bedded, and are characterized by the absence of clay bands and the presence of marine fossils; and that the Middle Bagshot is a well-marked series consisting of green sands and clays.

They claimed, in conclusion, that there was no reason for disturbing the old reading of the district, and that there was no evidence of an overlap of the Lower Bagshots by the Upper.

X. *Intelligence and Miscellaneous Articles.*

ABSOLUTE SPHERICAL ELECTROMETER. BY M. LIPPMANN.

THIS instrument consists essentially of an insulated metal sphere which is raised to the potential we desire to know. This sphere is constructed so as to divide into two hemispheres, which are movable in respect of each other, and which repel with a force equal to f when their system is electrified.

It can easily be shown that f and V are in the very simple ratio

$$f = \tfrac{1}{8} V^2.$$

In order to have V it is sufficient to measure f. This measurement might be made by several methods; I have adopted the following:—

In the first place, if the apparatus intended to measure f were external to the metal sphere, we should be obliged to put it so far off that its action had no disturbing influence on the distribution of the electricity. I have accordingly preferred to put the whole inside the electrified sphere itself, which is hollow.

One of the hemispheres is fixed; the other, which is movable, is suspended by a trifilar system, that is to say composed of three vertical wires of equal lengths. When repulsion is produced, the movable hemisphere can only be displaced parallel to itself; the three wires make then a small angle with their original vertical position; a is measured by a method of reflection by means of a mirror fixed to two of the wires [*], and seen through a small aperture. It will be seen that if p is the weight of the movable hemisphere, we have

$$f = p \tan a;$$

and therefore

$$p \tan a = \tfrac{1}{8} V^2.$$

Hence it is sufficient to know the weight p which is fixed; the value of the radius of the sphere is immaterial.

[*] The apparatus was constructed by MM. Breguet.

In a second copy of the same instrument, which I have the honour to place before the Academy, the system of two hemispheres is contained within a concentric spherical envelope, which is connected with the earth. This arrangement increases the sensitiveness of the instrument, and protects it from air-currents as well as from extraneous electrical influences.

If a and b are the radii of the two concentric spheres, we have the formula

$$p = \tfrac{1}{8}\, \frac{b^2}{(b-a)^2} V^2.$$

In this case we have $a = 3.9$ centims., $b = 4.92$, $p = 3.322$ grams. Hence if we place a millimetre scale at 1 metre from the rule, we have for the value of the deflection,

$$d = 0.00373 V^2.$$

If V is expressed in volts, we have

$$d = 0.0000140 V^3.$$

It is desirable to multiply optically the sensitiveness of the instrument by reading the deflections with an ocular which magnifies 15 to 50 times. This also diminishes in the same ratio the small deformation which the system of the spheres undergoes owing to the deflection.—*Comptes Rendus,* March 22, 1886.

SIMPLE DEMONSTRATION OF THE ELECTRICAL RESIDUE.
BY FR. STENGER.

If two strips of tinfoil are fastened at a suitable distance on a glass tube which has been evacuated as completely as possible, and are connected with the electrodes of an induction-coil, continuous electrical currents are formed in the interior, and the gas becomes intensely luminous. If, then, the two tinfoil rings are insulated, the tube becomes luminous at frequent intervals, and frequently for several minutes together. This doubtless arises from the formation of a residue on the sides of the glass. The experiment was made in a far more striking manner as follows :—In a glass tube, about 2 centim. in the clear, closed at one end, a thin metal foil was introduced so that it covered about half the inner side. A wire soldered to this plate was hermetically sealed in the tube, and was provided on the outside with a small knob. Opposite the metal cylinder a strip of tinfoil was fastened. This small Leyden jar was then rarefied as completely as possible, and hermetically sealed. If this was charged from a small electrical machine for ten or fifteen minutes the residual electricities could be discharged, producing a bright illumination in the tube. This method of rendering visible very small quantities of electricity might possibly be used in repeating in another form the experiments of Rowland and Nichols*, on the formation of the residue in quartz and calc-spar, which are of theoretical interest.—Wiedemann's *Annalen,* No. 6, 1886.

* Rowland and Nichols, Phil. Mag. [5] xi. p. 414 (1881).

XI. *The Law of Attraction amongst the Molecules of a Gas.*
By WILLIAM SUTHERLAND, *M.A., B.Sc.*[*]

MANY hypotheses have been advanced as to the law of
force prevailing among the molecules of a gas, chiefly
with a view to furnishing an explanation of part of the
departures from Boyle's and Charles's laws ; but, as they have
not started from a clear experimental basis, they have led to
no general result of any value. The clearest evidence which
has yet been given of the actual existence of attractive forces
amongst the molecules of gases was supplied by the difficult
experiments conducted by Thomson and Joule between the
years 1852 and 1862, and described in their papers in the
'Philosophical Transactions' on the "Thermal Effects of Fluids
in Motion." But of the two striking general conclusions
which they were able to draw from their experimental results,
no use has been made except that which they themselves made
in employing them to obtain an accurate expression of the
relations of volume, pressure, and temperature in the case of
air. The object of the present paper is to show that Thomson
and Joule's experiments prove that the molecules of a gas
attract one another with a force inversely proportional to the
fourth power of the distance between them, and directly
proportional to the product of their masses. It is hoped, too,
that the attention of physicists will be recalled to the power
of Joule's method in attacking the great problem of molecular
attractions in solids and liquids.

It will be remembered that Joule first attempted to get evidence of the molecular attractions or repulsions in a gas by allowing air to escape from under pressure in one vessel into vacuum in another, and by measuring the difference of temperature of the gas before and after. Stated as a general method, capable of application to all bodies to measure the alteration of their potential energy with the distance of their molecules, it is this :—Allow the body to pass instantaneously from one state to another without doing external work; the heat developed is the thermal equivalent of the change of potential energy. M. Edlund (*Poggendorf*, cxxvi.) made a study of some metal wires in this manner, but they were stretched, and of course the stretching had to be kept within the elastic limits. · The application of these experiments was limited to the verification of one or two thermodynamic relations. To obtain anything closer than a first approximation to the law connecting the potential energy and dimensions of bodies, it would be necessary to subject liquids and solids to pressures increasing to the greatest possible extent, and measure their change of potential energy, when released, by the thermal effect. This law once obtained, the deduction of the law of force would be a pure question of mathematical analysis.

Joule's first method not proving delicate enough for the case of gases, he joined Thomson in the series of experiments referred to above. Their method may briefly be described thus :—

Compressed gas was allowed to expand through a porous plug into the atmosphere. It was always brought to a constant measured temperature on the high-pressure side of the plug, and its temperature was taken on the low-pressure side. It was found to be cooled. However, part of the cooling effect could be traced to the departure of the gas from Boyle's law in this manner:—A volume v' of the compressed gas in expanding through the plug to volume v would have work $p'v'$ done on it by the gas behind (p' being the high pressure), while it would do work pv on the atmosphere in front. But in all gases except H, at about normal pressure and temperature, $pv > p'v'$. Hence, on the whole, the expanding gas does external work, and must accordingly draw on its supply of heat and get cooled. The thermal equivalent of $pv - p'v'$ at about 15° was calculated by Thomson and Joule from known data for air and CO_2, and was found to represent in the one case about a fourth, in the other about a third, of the actual cooling. The rest of the cooling effect is due to a gain of potential energy by the molecules at the expense of their heat ; in other words, the molecules of the expanding gas separate

against their mutual attractions. The two important general results obtained were :—

(1) The total cooling effect is directly proportional to the difference of the pressures on the two sides of the plug.

(2) The total cooling effect is inversely proportional to the square of the absolute temperature of the gas.

Although Thomson and Joule estimated the thermal equivalent of $pv - p'v'$ at 15°, they did not calculate its values at different temperatures and subtract them from the total cooling effect at the same temperatures, in order to get the parts of the cooling effect at these temperatures due to increase of the potential energy of the molecules. When this is done, the cooling effect due to increased potential energy, which we shall call θ, is, like the total cooling effect in (1), directly proportional to the difference of the pressures on the two sides of the plug, because at a given temperature $pv - p'v'$ is very nearly proportional to $p - p'$. But (2) does not now hold for θ. In its place we have this result, that the cooling effect due to increased potential energy is inversely proportional to the absolute temperature :—

$$\theta \propto \frac{p - p'}{\mathrm{T}}.$$

In obtaining the values of $pv - p'v'$ for air, Van der Waals's formula was employed,

$$\left(p + \frac{\cdot 0037}{v^2}\right)(v - \cdot 0026) = 1 \cdot 0011 \, (1 + \alpha t);$$

the unit of pressure being that of a metre of mercury, and the unit of volume that occupied by a kilogramme of gas at 0° C. and a pressure of one metre of mercury ; α is the coefficient of expansion, and t the temperature Centigrade.

Thomson and Joule give the cooling effects at different temperatures corresponding to difference of pressure of 100 inches, or 2·54 metres of mercury; so that to get values of $pv - p'v'$ corresponding to the same circumstances we must put $p = \cdot 76$ m., $p' = 3 \cdot 3$ m. A kilogramme of gas is supposed to pass through the plug. Changing to ordinary units and dividing by the mechanical equivalent of heat J and the specific heat of air s, we get finally the cooling effects $\frac{pv - p'v'}{\mathrm{J}s}$, due to departure from Boyle's law, as tabulated below.

The following table contains in the first column absolute temperatures, in the second the actual total cooling effects at the corresponding temperatures for a difference of pressure of 100 inches or 2·5 metres of mercury (these are taken from

G 2

Thomson and Joule's paper in the Phil. Trans. 1862); in the third the calculated cooling effect $\dfrac{pv - p'v'}{Js}$; in the fourth the value of θ or actual cooling effect *minus* $\dfrac{pv - p'v'}{Js}$; and in the fifth the products $T\theta$.

Air.

Absolute temperature, T.	Total actual cooling effect.	Cooling effect, $\dfrac{pv - p'v'}{Js}$.	Difference of the two cooling effects, θ.	Product, $T\theta$.
$273 \cdot \overset{\circ}{0}$	$\cdot 9\overset{\circ}{2}$	$\cdot 32\overset{\circ}{0}$	$\cdot 60\overset{\circ}{0}$	164
$280 \cdot 1$	$\cdot 88$	$\cdot 294$	$\cdot 586$	164
$312 \cdot 5$	$\cdot 75$	$\cdot 183$	$\cdot 567$	177
$365 \cdot 8$	$\cdot 51$	$\cdot 046$	$\cdot 464$	166

Thus within a range of nearly 93° the product $T\theta$ is practically constant, showing that the cooling effect due to increase of molecular potential energy is inversely proportional to the absolute temperature of the gas. The erratic number 177 is eliminated from the following table, in which, instead of the actual, experimental total cooling effects are used, those which Thomson and Joule calculated in the light of the fact that variation of the total cooling as the inverse square of the absolute temperature was the clear meaning of their experiments as a whole :—

Absolute temperature, T.	Calculated actual cooling effect.	Cooling effect, $\dfrac{pv - p'v'}{Js}$.	Difference of the two cooling effects, θ.	Product, $T\theta$.
$273 \cdot \overset{\circ}{0}$	$\cdot 9\overset{\circ}{2}$	$\cdot 32\overset{\circ}{0}$	$\cdot 60\overset{\circ}{0}$	164
$280 \cdot 1$	$\cdot 87$	$\cdot 294$	$\cdot 576$	161
$312 \cdot 5$	$\cdot 70$	$\cdot 183$	$\cdot 517$	162
$365 \cdot 8$	$\cdot 51$	$\cdot 046$	$\cdot 464$	166

With the relation $\theta \propto \dfrac{p - p'}{T}$ thus established, it is not difficult to see that it means that the attraction between any two molecules of the gas is proportional to the product of their masses, and inversely proportional to the fourth power of the distance between them.

For the mutual potential energy of the two molecules is inversely proportional to the cube of the distance between

them; or we may say that the potential of a particle of mass m at a point distant r from it is given by the equation

$$V = A\frac{m}{r^3}.$$

Let us assume that the constant A is of such magnitude that V becomes negligible for values of r greater than a certain length R, which corresponds to the radius of the sphere of action considered in most molecular theories, R being a large multiple of the distance between a molecule and its nearest neighbour and at the same time small compared to sensible distances. R is of the order of magnitude of, say, the thickness of a capillary film. Under these circumstances we can consider each molecule as uniformly distributed through the small region of space round it, which may be said to belong to it. Then any one molecule P may be supposed to be gathered into a particle at its centre, leaving the space which belongs to it in the form of a spherical vacuum, while all the other molecules have been spread out around it into a continuous matter of uniform density ρ. To find the potential of a finite mass of gas at the centre of P, let us describe a cone of small solid angle w with its vertex at P, and terminating at the boundary of the gas AA' ; it cuts off the small area aa' on the surface of the vacuous sphere. Then for the potential at P of
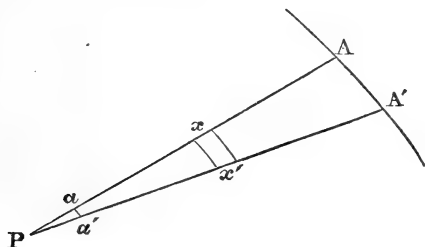
any element xx' distant r from P and of thickness dr we have $A\frac{\rho w r^2 dr}{r^3}$; and therefore for the whole potential of the frustrum aa' $A'A$,—

$$A\rho w\int_{r_1}^{R_1}\frac{dr}{r} = A\rho w \log\frac{R_1}{r_1},$$

where $Pa = r_1$ and $PA = R_1$.

Let $R_1 = nL$, where n is a large number so that L is a small but sensible length.

Then the above becomes $A\rho w \log \dfrac{nL}{r_1}$,

$$i.\ e.\ A\rho w \log n + A\rho w \log \frac{L}{r_1};$$

but r_1 is so excessively small in comparison with the sensible length L that n is negligible in comparison with $\dfrac{L}{r_1}$. Hence we may write $A\rho w \log \dfrac{L}{r_1}$ as equivalent to the above.

Hence for the potential of the finite mass of gas round P_1 at the centre of P we can take that of the sphere of matter of radius L, which is,

$$4\pi A\rho \log \frac{L}{r_1}.$$

Hence for the mutual potential energy of the particle P and the whole mass of gas, we have

$$4\pi A m\rho \log \frac{L}{r_1};$$

and for the total potential energy of n molecules, leaving out of count those so near the boundary that a sphere of radius L cannot be described about them so as to lie wholly in the matter under consideration, we have,

$$2\pi A n m\rho \log \frac{L}{r_1},$$

changing from the numerical coefficient 4 to 2, because we must not count the mutual potential energy of any two particles twice over.

If now the mass of gas is allowed to expand (in Thomson and Joule's experiments it expanded by various amounts up to six times the original volume), the value of $\log \dfrac{L}{r_1}$ remains practically constant, and the new value of the total potential energy is

$$2\pi A n m\rho' \log \frac{L}{r_1},$$

where ρ' is the density of the gas after expansion.

Therefore the change of potential energy is proportional to

$$M\ (\rho - \rho'),$$

M being the mass of gas.

Now the cooling effect corresponding to this will be obtained by dividing the above by JMs, where s is the specific heat of

the gas. Thus the cooling effect is proportional to

$$\frac{\rho - \rho'}{Js}.$$

If T is the absolute temperature of the gas and p and p' the pressures corresponding to the densities ρ and ρ',

$$\frac{\rho - \rho'}{Js} \text{ is proportional to } \frac{p - p'}{Ts}.$$

In the case of air s is practically constant, so that the theoretical cooling is directly proportional to the difference of pressures and inversely to the absolute temperature.

Thus the hypothesis of a force attracting according to the law of the inverse fourth power and the product of the masses yields the two results deduced from the experimental data.

It may be worth while mentioning that if the case is worked out for a very long cylinder of matter, attracting according to the Newtonian law, treated as a very prolate spheroid and expanded into another cylinder of the same section treated also as a spheroid, results in accordance with the above experimental results may be obtained, but with a third result, that the cooling effect would be proportional to the sectional area of the cylinder. Thus if the time ever comes when it will be practicable to look for the part of the cooling effect due to the mutual gravitation of the molecules, it will be found as a small fraction of the whole cooling effect, varying with the sectional area of the plug.

The only other gas on which Thomson and Joule conducted a sufficiently extended series of experiments to obtain definite results was CO_2. They were able to enunciate the same two general results as for air, only the total cooling effects were not so accurately proportional to the inverse square of the temperature. To evaluate the thermal equivalent of $pv - p'v'$ at different temperatures for CO_2, Clausius' formula is used,

$$p = 19 \cdot 273 \frac{T}{v - \cdot 000426} - \frac{5533}{T(v + \cdot 000494)^2}.$$

The unit of pressure is that of a kilogramme per square metre, and v is the volume in cubic metres of a kilogramme of CO_2.

But, in the first place, the formula shows how at a given temperature the value of $pv - p'v'$ is very nearly proportional to $p - p'$, so that, as in the case of air, we can assert that the cooling effect due to increase of molecular potential energy is proportional to the difference of pressure on the two sides of the plug.

In passing from the value of $pv - p'v'$ to the cooling effect $\dfrac{pv - p'v}{\mathrm{J}s}$, which corresponds to it, account has been taken of the variation of s, the specific heat of CO_2, with temperature. E. Wiedemann's determinations ($\cdot$1952 at $0°$ C., $\cdot$2169 at $100°$, $\cdot$2837 at $200°$) were adopted, the values at intermediate temperatures being obtained by interpolation. On reverting to the theoretical conclusion, it will be seen that the cooling is to be proportional to $\dfrac{p - p'}{\mathrm{T}s}$; and as s varies with the temperature, it will be necessary to test the theory by seeing whether the product $\mathrm{T}\theta s$ (for the constant value 100 inches of mercury for $p - p'$) is constant.

In the first of the following tables the total actual cooling effects are Thomson and Joule's experimental numbers (Phil. Trans. 1862); in the second the calculated total cooling effects were obtained by them on the supposition that the cooling effects were inversely proportional to the square of the absolute temperature.

CO_2.

Absolute temperature, T.	Actual total cooling effect.	Cooling effect, $\dfrac{pv - p'v'}{\mathrm{J}s}$.	Difference of the two cooling effects, θ.	Product, $\mathrm{T}\theta s$.
$273 \overset{\circ}{\cdot} 0$	$\overset{\circ}{4} \cdot 64$	$\overset{\circ}{1} \cdot 43$	$\overset{\circ}{3} \cdot 21$	171
308·6	3·41	1·04	2·37	148
327·0	2·95	·89	2·0	141
370·5	2·14	·63	1·51	120

Absolute temperature, T.	Calculated total cooling effect.	Cooling effect, $\dfrac{pv - p'v'}{\mathrm{J}s}$.	Difference of the two cooling effects, θ.	Product, $\mathrm{T}\theta s$.
$273 \overset{\circ}{\cdot} 0$	$\overset{\circ}{4} \cdot 64$	$\overset{\circ}{1} \cdot 43$	$\overset{\circ}{3} \cdot 21$	171
308·6	3·63	1·04	2·59	162
327·0	3·23	·89	2·34	158
370·5	2·52	·63	1·89	152

In the first of the above tables for CO_2, the products $\mathrm{T}\theta s$ show decided enough departure from constancy; in fact $\mathrm{T}^2\theta s$ would be nearly constant. The departure may be taken as showing the great value which similar experiments conducted on vapours compressed nearer and nearer to liquefaction and

then allowed to expand might possess. For in view of the facts that at 0° C. CO_2 liquefies under a pressure of about 40 atmospheres, while at 45° there is required 100 atmospheres to liquefy it, and that the pressure on the high-pressure side of the plug, in the experiments from which the above numbers were derived, reached 6 atmospheres, it becomes apparent that at the low temperatures the forces which are ultimately to produce cohesion in the liquid are hardly likely to be so closely represented by the monomial expression $\frac{A}{r^4}$ as at the high temperatures. In fact, regarding the ultimate law of the action of one particle on another at any distance as a function of r of the form $f\left(\frac{1}{r}\right)$ or $f\left(\frac{1}{r^2}\right)$, and considering the law of gravitation as simply the first term of the expansion of the latter in ascending powers of $\frac{1}{r^2}$, which expresses the action accurately enough within the limits of astronomical distances, we may look upon Thomson and Joule's experiments on air as showing how the second term, involving the inverse fourth power of r, becomes appreciable at very small distances ; in the case of CO_2 we may regard the above table as showing how the term $\frac{1}{r^6}$ may begin to be appreciable, and how perhaps at still smaller distances still higher terms may appear and become predominant in producing cohesion and elasticity.

There remains one application of our theory which throws an interesting light on a fact to which Thomson and Joule drew attention more than once as being very remarkable. When a mixture of the two gases, CO_2 and air, is expanded through a plug, it might be expected that each would contribute its proportion of cooling effect according to its own amount and its thermal capacity. But such is far from being the case. Indeed, experiment showed that the cooling effect for pure O is greater than for pure N, and yet in air and other mixtures of the two gases the cooling effect is *less* than in *either* of the constituents under the same circumstances.

Let V_1 be the volume of a mixture of two gases before expansion, V_2 the volume after. Let V_{A_1}, V_{B_1}, be the volumes of the two constituent gases A and B before expansion ; V_{A_2}, V_{B_2}, the volumes after. Suppose that there are a molecules of A and b molecules of B in the mass under consideration. We must first make a hypothesis as to the action of a molecule of A on a molecule of B. If the mutual potential of two

molecules of A at distance r apart is $\dfrac{\mathrm{A}m^2}{r^3}$, and of two molecules

of B is $\dfrac{\mathrm{B}m'^2}{r^3}$, we will assume that the mutual potential of a

molecule of A and one of B at distance r is $\mathrm{C}\dfrac{\sqrt{\mathrm{AB}}\,m\,m'}{r^3}$,
where C is a constant.

Then for the mutual potential energy of all the molecules of A before expansion we have an expression

$$2\pi\mathrm{A}\;am\rho_{\mathrm{A}_1}\;\log\frac{\mathrm{L}}{r_1},$$

where ρ_{A_1} means the density of the gas A when its a molecules are distributed through a volume V_1. Similarly for the mutual potential of the molecules of B before expansion we have

$$2\pi\mathrm{B}\;bm'\rho_{\mathrm{B}_1}\;\log\frac{\mathrm{L}}{r_1},$$

the value of r_1 being the same in each expression, because, according to Avogadro's law, the molecules of different gases under the same circumstances own equal volumes of space. Leaving the quantity L of the same value in both expressions, amounts to asserting that the molecular forces in the two gases are quantities of the same order of magnitude. For the mutual potential energy of a molecule of A and one of B before expansion we have

$$4\pi\mathrm{C}\;\sqrt{\mathrm{AB}}\;m'\rho_{\mathrm{A}_1}\log\frac{\mathrm{L}}{r_1}\;;$$

and, therefore, for the mutual potential of the a molecules of A and the b molecules of B we have

$$4\pi\mathrm{C}\;\sqrt{\mathrm{AB}}\;bm'\rho_{\mathrm{A}_1}\log\frac{\mathrm{L}}{r_1}.$$

But by proceeding in the other order, that is by writing down the mutual potential of the b particles of B and one of A and then summing for the a particles, we would obtain

$$4\pi\mathrm{C}\;\sqrt{\mathrm{AB}}\;am\rho_{\mathrm{B}_1}\log\frac{\mathrm{L}}{r_1}.$$

Thus for the total energy of the mixed gases before expansion we have the expression (omitting common constants)

$$\mathrm{A}am\rho_{\mathrm{A}_1}+\mathrm{B}b\,m'\rho_{\mathrm{B}_1}+2\mathrm{C}\;\sqrt{\mathrm{AB}}\;bm'\rho_{\mathrm{A}_1}\;;$$

and after expansion,

$$\mathrm{A}am\rho_{\mathrm{A}_2}+\mathrm{B}bm'\rho_{\mathrm{B}_2}+2\mathrm{C}\;\sqrt{\mathrm{AB}}\;bm'\rho_{\mathrm{A}_2},$$

ρ_{A_2} denoting the density of the gas A when its a molecules are distributed through a volume V_2.

Then for the increase of potential due to expansion we have

$$A am(\rho_{A_1}-\rho_{A_2}) + B\, bm'(\rho_{B_2}-\rho_{B_1}) + 2C\sqrt{AB}\, bm'(\rho_{A_2}-\rho_{A_1}).$$

Now the last term in this has the same value as

$$2C\sqrt{AB}\, am(\rho_{B_2}-\rho_{B_1});$$

so that it may be replaced by the square root of the product of the two, namely

$$2C\sqrt{AB\, am\, bm'(\rho_{B_2}-\rho_{B_1})(\rho_{A_2}-\rho_{A_1})}.$$

Hence for the increase of potential energy we have the sum of the two expressions $A am(\rho_{A_2}-\rho_{A_1})$; $B bm'(\rho_{B_2}-\rho_{B_1})$; and C times twice the product of their square roots.

Now suppose that V_1 is the volume of the mixture at a pressure P_1; V_2 at a pressure P_2; the temperature T being the same in each case. Then ρ_{A_1} is to the density of the gas A at pressure P_1 in the ratio $\dfrac{V_{A_1}}{V_1}$, and ρ_{A_2} is to the density of the gas A at pressure P_2 in the ratio $\dfrac{V_{A_2}}{V_2}$, which is equal to the previous ratio. Thus the term $A am(\rho_{A_2}-\rho_{A_1})$ may be written

$$A am\frac{V_{A_1}}{V_1}(\rho'_A-\rho_A),$$

where ρ_A, ρ'_A represent the densities of A at pressures P_1 and P_2. But this is $\dfrac{V_{A_1}}{V_1}$ times the gain of potential of a mass am of the gas A escaping from under pressure P_2 to pressure P_1; or, if we call θ_A the cooling effect for A corresponding to P_2-P_1, we may write it

$$J s_A \rho_A V_{A_1}\frac{V_{A_1}}{V_1}\theta_A,$$

where s_A is the specific heat of A.

For the other terms in the gain of potential by the mixed gases we can write corresponding expressions, and get for the result,

$$J s_A \rho_A \frac{V_{A_1}^2}{V_1}\theta_A + J s_B \rho_B \frac{V_{B_1}^2}{V_1}\theta_B + 2CJ\sqrt{s_A \rho_A \frac{V_{A_1}^2}{V_1}\theta_A\ \ s_B \rho_B \frac{V_{B_1}^2}{V_1}\theta_B}.$$

To obtain the cooling effect corresponding to this we must first divide by J, and then by the thermal capacity of the

mass operated on, which is
$$s_A \rho_A V_{A_1} + s_B \rho_B V_{B_1}.$$

Therefore for the cooling effect θ of the mixture we have

$$\theta = \frac{s_A \rho_A \dfrac{V_{A_1}^2}{V_1}\theta_A + s_B \rho_B \dfrac{V_{B_1}^2}{V_1}\theta_B + 2C\sqrt{s_A \rho_A \dfrac{V_{A_1}^2}{V_1}\theta_A \; s_B \rho_B \dfrac{V_{B_1}^2}{V_1}\theta_B}}{s_A \rho_A V_{A_1} + s_B \rho_B V_{B_1}}.$$

To show with this formula how the cooling for a mixture of O and N may be less than for either of the gases alone under the same circumstances, let us suppose that θ is less both than θ_A and θ_B; we will see whether the supposition leads to a possible or impossible conclusion.

Let us denote
$\dfrac{V_{A_1}}{V_1}$ by D, and $\dfrac{V_{B_1}}{V_1}$ by D', remembering that $D + D' = 1$;

also $s_A \rho_A$ by α, $s_B \rho_B$ by β.

Then the supposed inequalities become

$$\alpha D^2 \theta_A + \beta D'^2 \theta_B + 2CDD'\sqrt{\alpha\beta\theta_A\theta_B} < (\alpha D + \beta D')\theta_A,$$

$$\beta D'^2 \theta_B + \alpha D^2 \theta_A + 2CDD'\sqrt{\alpha\beta\theta_A\theta_B} < (\alpha D + \beta D')\theta_B;$$

$$\therefore \; \alpha D\theta_A(D-1) + \beta D'^2\theta_B - \beta D'\theta_A + 2CDD'\sqrt{\alpha\beta\theta_A\theta_B} < 0,$$

or

$$-\alpha DD'\theta_A + \beta D'^2\theta_B - \beta D'\theta_A + 2CDD'\sqrt{\alpha\beta\theta_A\theta_B} < 0;$$

$$\therefore \; -\alpha D\theta_A + \beta D'\theta_B - \beta\theta_A + 2CD\sqrt{\alpha\beta\theta_A\theta_A} < 0.$$

Similarly

$$-\beta D'\theta_B + \alpha D\theta_A - \alpha\theta_B + 2CD'\sqrt{\alpha\beta\theta_A\theta_B} < 0;$$

$$\therefore \text{ adding } -\beta\theta_A - \alpha\theta_B + 2C(D+D')\sqrt{\alpha\beta\theta_A\theta_B} < 0.$$

If $C = 1$, this becomes, since $D + D' = 1$,
$$-(\sqrt{\beta\theta_A} - \sqrt{\alpha\theta_B})^2 < 0,$$

which is possible. If C is less than 1, the inequalities can also still exist together.

Thus that the cooling effect for a mixture of two gases should prove less than that for either of the constituents has been shown to be a possible consequence of the theory of molecular attractions.

By means of Thomson and Joule's experimental numbers for mixtures of air and CO_2 in different proportions, we propose finally to calculate the values of C obtainable from the cooling

effects in different mixtures and see whether they agree ; that is, whether our theory can be tested by its power to explain with any exactness a very peculiar experimental fact.

There are no systematized experimental results by means of which the value of pv for different mixtures of air and CO_2 at different temperatures could be obtained directly ; but the following argument will show that we can make use of our previous numbers for the values of $pv - p'v'$ at different temperatures in the case of air and CO_2, to deduce the numbers for any mixture of air and CO_2 at the same temperatures. Let V_1 be the volume of a mixture of the two gases at a certain temperature and a pressure P_1; as before, let V_{A_1}, V_{B_1} be the volumes which the constituent gases in V_1 would occupy if separated at pressure P_1. Let W_1 be the potential energy of the mixed gases W_{A_1}, W_{B_1} of the two separated gases ; then actually to separate the two gases will require work,

$$W_1 - W_{A_1} - W_{B_1}.$$

Expand the separated gases to a condition represented by suffix 2, just as they were expanded in Thomson and Joule's experiments, that is without doing external work other than that corresponding to the values of $pv - p'v'$ for each gas ; thus each of the separated gases would be cooled by the amount $\dfrac{pv - p'v'}{Js}$ previously calculated, and each would be cooled by its respective amount θ_A or θ_B, on account of the separation of molecules ; so that altogether the gain of potential energy during the expansion will be the sum of $Js_A\theta_A$ and $Js_B\theta_B$ and the two corrections $pv - p'v'$. Thirdly, allow the gases to diffuse into one another. In this case the work required will be

$$-W_2 + W_{A_2} + W_{B_2}.$$

Hence the total gain of potential energy by the mixed gases on expanding from volume V_1 to V_2 is

$$W_1 - W_{A_1} - W_{B_1} - W_2 + W_{A_2} + W_{B_2} + Js_A\theta_A + Js_B\theta_B$$
$$+ (P_1V_{A_1} - P_1V_{A_2}) + (P_1V_B - P_2V_{B_2}) ;$$

but $\qquad W_{A_1} - W_{A_2} = Js_A\theta_A ; \quad W_{B_1} - W_{B_2} = Js_B\theta_B.$

So that the total gain reduces to

$$W_1 - W_2 + (P_1V_{A_1} - P_2V_{A_2}) + (P_1V_{B_1} - P_2V_B),$$

and this corresponds to the total actual cooling effect observed by Thomson and Joule. The cooling effect denoted above by θ is the equivalent of $W_1 - W_2$; to obtain θ then from the

experimental numbers we have to subtract from them the cooling effects corresponding to the terms $(P_1 V_{A_1} - P_2 V_{A_2})$.

Given that a volume V_{A_1} of air is cooled by $\dfrac{P_1 V_{A_1} - P_2 V_{A_2}}{J s_A}$,

and that a volume V_{B_1} of CO_2 is cooled by $\dfrac{P_1 V_{B_1} - P_2 V_{B_2}}{J s_B}$, we have for the cooling effect in the mixture :—

$$\frac{1}{J} \left(\frac{(P_1 V_{A_1} - P_2 V_{A_2}) V_{A_1} \rho_A + (P_1 V_{B_1} - P_2 V_{B_2}) V_{B_1} \rho_B}{V_{A_1} \rho_A s_A + V_{B_1} \rho_B s_B} \right).$$

In this manner the $pv - p'v'$ part of the cooling effect of the mixed gases has been calculated from the previously found values for pure air and CO_2.

The table below contains in the first column the percentages of the two gases in the particular mixture ; the second contains the temperature (absolute) T at which the gas escaped ; the third contains the total actual cooling effect for a difference of pressure of 100 in. or 2·54 metres of mercury, observed by Thomson and Joule (Phil. Trans. 1862) ; the fourth contains the calculated values of the above expression for the cooling effect to be subtracted; the fifth contains the cooling effect θ obtained by subtracting the numbers in the fourth column from those in the third ; while the sixth contains the values of C obtained by substituting the corresponding values of θ in the equation for θ.

Mixtures of Air and CO_2.

Percentage composition of mixture.	Absolute temperature, T.	Total actual cooling effect.	Cooling effect to be subtracted.	θ.	C.
68 air 32 CO_2 ········	280°	1·76°	·69°	1·07°	·72
89 air 11 CO_2 ········	280	1·17	·45	·72	·76
62 air 38 CO_2 ········	280	1·86	·76	1·1	·57
68 air 32 CO_2 ········	323	1·29	·49	·8	·8
88 air 12 CO_2 ········	323	·88	·33	·55	·6
57 air 43 CO_2 ········	364	1·1	·36	·74	·73

The agreement between these values of C calculated from different mixtures at different temperatures is very fair, especially if the value ·57 is left out of the count as being evidently affected by some error in the experiment from which it is deduced ; because, if 1°·07 is correct for the first value of θ,

1·1 can hardly be right when the percentage of CO_2 has been increased from 32 to 38. Thus we can regard the experiments on mixed gases as furnishing confirmation of the truth of the law of the inverse fourth power for the attractions between molecules of gas.

The value of C which we have obtained for air and CO_2 (mean 17) throws an interesting light on a certain aspect of the phenomenon of diffusion. Suppose a volume V_A of a gas A containing a molecules, and a volume V_B of another gas B containing b molecules, separated by an infinitely thin partition, then the potential energy of the two masses of gas is (leaving out common factors)

$$A a m \rho_A + B b m' \rho_B.$$

When the gases are mixed together without change of pressure we can see from what has gone before that the potential becomes

$$A a m \rho_A \frac{V_A}{V_A + V_B} + B\, bm' \rho_B \frac{V_B}{V_A + V_B} + 2C\sqrt{AB\, ambm'\, \frac{V_A V_B}{(V_A + V_B)^2}}.$$

Subtracting this from the previous expression, we get

$$A a m \rho_A \frac{V_B}{V_A + V_B} + B\, bm' \rho_B \frac{V_A}{V_A + V_B} - 2C\sqrt{AB\, ambm'\, \frac{V_A V_B}{(V_A + V_B)^2}}.$$

If $C = 1$ this is a complete square, and therefore necessarily positive; therefore when $C = ·7$ the expression is also positive, and for all values of C less than 1 it must be positive; that is, the potential energy of the molecules diminishes by diffusion. Hence we may regard diffusion as partly due to the tendency of the molecules of the mixing gases to obey the dynamical principle that a position of stable equilibrium is a position of minimum potential energy. Diffusion is motion towards the position of stable equilibrium for the two gases.

However, the kinetic factor in the diffusion of gases is so predominant that this aspect is not of much importance. But in the case of liquids it is otherwise, and the form of our last expression suggests how the tendency of some liquids to mix and of others to refuse to mix may depend on the magnitude of a coefficient like C. Indeed, the study of the cooling effect of liquids and mixtures of liquids escaping from under pressure affords a splendid field for experimental inquiry. This paper will have possessed some value if it draws the attention of those who have facilities for such a research, to a field whose further exploration on the tracks of the pioneers must open valuable ground for Physical Science.

Melbourne, April 1886.

XII. *New Instrument for continuously recording the Strength and Direction of a Varying Electric Current. By* R. SHIDA, *M.E., Professor of Natural Philosophy in the Imperial College of Engineering, Tokio, Japan.*

[Plates II. & III.]

To Sir William Thomson, F.R.S., LL.D., &c.

DEAR SIR WILLIAM,

I ENCLOSE herewith a paper which I have just drawn up and which is a description of a new instrument I have devised and constructed for continuously recording the strength and direction of a varying electric current. My chief aim in designing such an instrument was to use it for making observations of both regular and irregular variations of earth-currents which are present in the telegraph-wires of this country, just as they are present in the telegraph-wires of any other country. The importance of carrying on careful observations of earth-currents has been felt more and more since you showed it before the Society of Telegraph Engineers and Electricians, about eleven years ago, in your presidential address. Indeed, so great an importance is now attached to such observations, that it was one of the main subjects discussed by the International Electric Congress which met at Paris last year. Now, since both regular and irregular earth-currents are so variable, that their strength and direction change, not only from day to day, but from hour to hour, or from minute to minute, or even from second to second, observations will be of very little value unless they are continuously made; hence the importance of a method of continuously registering the strength and direction of varying electric currents. The photographic method, such as is used in the Kew Observatory, is, of course, very satisfactory and accurate. But this method, besides requiring an elaborate arrangement of several pieces of apparatus, has a serious disadvantage, namely, that the observations must be made in a dark room. I have therefore felt for a long time the want of a method which, though not so accurate as the photographic method, is simple and convenient. It was thus that I was led to devise the apparatus described in the accompanying paper.

As will be seen from the description given in the paper, the galvanometer-part of the apparatus is, in the main, the same as that of the more recent one of your Siphon Recorders; that is to say, a coil containing a great number of turns of fine wire is suspended in a strong magnetic field produced by

permanent ·magnets. There is, however, one point in the apparatus which is quite new, at least quite new to my knowledge : that is, that the advantage is taken of a singular property of matter, " surface-tension of liquids." We know very well that a mercury-drop is often used for the purpose of making and breaking an electric circuit. But nobody has used a water-drop or an acidulated-water drop for the same purpose. The advantage of a water-drop over a mercury-drop, when employed for opening and closing an electric circuit, is that the former offers a far smaller resistance than the latter to the moving body which comes in contact with it. Now, in the instrument I am speaking of, a water-drop or, what is equivalent to it, a thin column of water drawn up between two narrow plates partly immersed in water, is used for the purpose of making and breaking the circuit, as will be seen from the description given in the paper.

For the further details of the apparatus I ask you to be good enough to refer to the paper itself.

The Instrument, I might mention, may of course be used as a " coulombmeter," because since in the paper ribbon, on which a record is obtained, the abscissas represent times and the ordinates represent currents, the area included by (the abscissa) the line of no current, and the ordinates corresponding to any two times and the curve of current, represents the quantity of electricity passed through the apparatus during the interval between the two times.

<div align="center">* * * *</div>

<div align="right">R. SHIDA.</div>

ONE of the principal subjects discussed by the International Electric Congress held in Paris in 1884, was that of Earth-currents ; and the result of the Congress as regards earth-currents was " that the Conference expresses the wish that observations of earth-currents be pursued in all countries." This resolution, together with the others, has been communicated to the various Governments ; and our Government having conformed to the wish of the Conference, it was decided that the observations of earth-currents be made by the Telegraph Department, in which I am a chief engineer. It thus devolved on me to take the subject up. A little consideration, suggested by the results of preliminary observations I have made of earth-currents, revealed to me that in order to carry out systematic observations of earth-currents, which, from time to time, vary in strength and direction, it is almost necessary, or at least extremely convenient, to have at our disposal a simple instrument which will continuously record

the strength and direction of a varying electric current.   In view of this, I have designed and constructed an instrument the description of which I have now the pleasure of communicating.   In order that an instrument may continuously record a varying electric current, it is necessary that it should fulfil the two following conditions :—

1. That the motion of the needle of the galvanometer (which is a part of the instrument) be such that the same position of the needle always corresponds to the same strength of current, that is to say that the motion be non-oscillatory.

2. That the position of the needle of the galvanometer at any moment be recorded.

I shall first explain generally how these two conditions are satisfied in the new instrument I am going to describe.

As regards the first condition.   This condition is satisfied by having a galvanometer whose needle consists of a coil of fine wire suspended in a powerful magnetic field, after the manner of the Siphon Recorder of Sir William Thomson.   It is easy to show mathematically that in the case of an ordinary galvanometer, which consists of a magnetic needle suspended inside, or in the neighbourhood of a coil of wire, this condition cannot conveniently be fulfilled without diminishing its sensibility.   On the other hand, in the case of a galvanometer consisting of a coil hung in a strong magnetic field as above described, it is easy to obtain a great sensibility and, at the same time, a non-oscillatory motion of the needle, as will be seen from the following investigations :—

Let α be the angle of deflection of the coil at any time t, and let T be its period of oscillation when no current is circulating through it ; then we have for the equation of the motion

$$\frac{d^2\alpha}{dt^2} + \frac{4\pi^2}{T^2} = 0.$$

But when a current circulates through the coil, the equation of the motion will be altered owing to a retardation of the motion due to the current induced in the coil.   Let us consider the magnitude of this retardation.   If I be the intensity of the magnetic field which the coil occupies at time t, A the area included in all the turns of the coil, and if we neglect the self-induction of the coil on itself (which I think we can confidently do) ; then, plainly, N, the number of lines of force which pass through the coil at time t, is

$$N = IA \sin \alpha ;$$

hence

$$\frac{dN}{dt} = IA \cos \alpha \frac{d\alpha}{dt}.$$

But $\dfrac{d\mathrm{N}}{dt}$ is the E.M.F. due to the inductive action ; hence, if R be the resistance of the circuit and α be small, the current induced in the coil at any time t, is approximately

$$c = \frac{\mathrm{IA}}{\mathrm{R}} \cdot \frac{d\alpha}{dt}.$$

Now the couple or torque due to the action of the field on the circuit is $c\mathrm{IA}$; and therefore the retardation of the angular velocity of the coil at any time t is

$$\frac{\mathrm{I}^2\mathrm{A}^2}{\mu\mathrm{R}} \cdot \frac{d\alpha}{dt},$$

where μ is the moment of inertia of the coil. Hence we have, for equation of the motion of the coil,

$$\frac{d^2\alpha}{dt^2} + \frac{\mathrm{I}^2\mathrm{A}^2}{\mu\mathrm{R}} \cdot \frac{d\alpha}{dt} + \frac{4\pi^2}{\mathrm{T}^2}\alpha = 0.$$

The motion represented by this equation will be oscillatory or non-oscillatory according as $2\pi/\mathrm{T}$ is greater or less than $\mathrm{I}^2\mathrm{A}^2/2\mu\mathrm{R}$, so that in order to make the motion of the coil non-oscillatory, all that is necessary is to have the magnetic field so strong that $\mathrm{I}^2 > \dfrac{4\pi}{\mathrm{T}} \cdot \dfrac{\mu\mathrm{R}}{\mathrm{A}^2}.$

Now as regards the second condition. The method commonly used of recording the motion of the needle of a galvanometer is the photographic method, which is, undoubtedly, very satisfactory. But this method, besides requiring an elaborate arrangement of several pieces of apparatus, has a serious disadvantage, namely that the observations must be made in a darkened room. The method adopted in the new instrument is one which, though, perhaps, not so accurate as the photographic method, possesses the advantage of being very simple and convenient. In this method, which may be called the electrical method, there are several electrical circuits, each of which is closed when, and only when, the coil or the needle comes to a certain definite position corresponding to it, and each circuit, when closed, makes a mark on a moving paper ribbon chemically prepared, somewhat in the same way as in the Bain's Telegraphs. If the coil turns round in one direction, it successively closes those circuits which make marks on one side of the centre of the ribbon, and if in the opposite direction, those circuits which make marks on the other side ; and further, the distance of the mark from the centre of the ribbon is greater or less according as the turning round of the coil is greater or less.

How these electrical circuits are exactly arranged will be seen later on with reference to the diagrams of the actual instrument.   At present it suffices to explain how the coil or needle closes each electrical circuit separately, and without its motion being being checked or impeded.   This is effected by taking advantage of one of the well-known properties of matter, " surface-tension of liquids."   When a capillary tube is partly immersed in a liquid which wets the tube, like water, the liquid ascends in the tube, and the smaller the diameter of the tube the greater the height to which the liquid ascends, and *vice versâ.*   In fact, it can be shown that if θ be the angle of capillarity, r the radius of the tube, w the weight of unit volume of the liquid, T the surface-tension per unit length of the liquid in contact with air, then h, the height to which the liquid rises, is

$$h = \frac{1}{r} \cdot \frac{2 \mathrm{T} \cos \theta}{w}.$$

But the liquid is drawn up in the same way in the space between two parallel plates.   In this case, if d be the distance between two plates, then

$$h = \frac{1}{d} \cdot \frac{2 \mathrm{T} \cos \theta}{w};$$

which shows that the height to which a liquid rises between two parallel plates is equal to the height to which it rises in a tube whose radius is equal to the distance between the plates.

Imagine now that there is a large number of capillary arrangements, each consisting of two very narrow plates standing in a vessel containing water at small distances from one another, and arranged in an arc of a circle, while the needle of the galvanometer is disposed in such a manner that, as it turns round, it successively comes in contact with the column of water drawn up between the plates of each of those capillary arrangements and thus closes several circuits in order ; or else, that there is one such capillary arrangement, while the needle carries a large number of points so disposed that, when it turns round, these points successively come in contact with the column of water in the capillary arrangement, and thus close several circuits in order.   Either of these arrangements affords us the means of closing each circuit separately, and without the motion of the needle being checked.   In the new instrument the latter plan is used, as will be more clearly seen with reference to the diagrams (Plates II. and III.).

Having now explained briefly the principles upon which

the action of the apparatus depends, I shall proceed to describe
the construction and action of the apparatus. Fig. 1 (Pl. II.)
shows the general view of the apparatus ; while figs. 2, 3, and 4
(Pl. III.) shows the details of the arrangement of the coil, mag-
net, &c. N and S are the poles of a powerful horseshoe magnet
consisting of a bundle of square bar-magnets made of very hard-
tempered steel. Between the poles N and S there is sus-
pended, by means of a fine silk thread, a coil (C), which contains
a great many turns of a very fine insulated wire, and whose
plane is at right angles to the line joining the two poles of
the magnet; *m* is a piece of soft iron fixed inside the coil,
nearly filling, but nowhere touching, it, and serves to intensify
the magnetic field in which the coil hangs. When an electric
current passes, the coil tends to turn round a vertical axis in
one direction, or in the opposite direction, according as the
current is positive or negative. The two weights, *w, w,*
hanging from the coil can slide up and down the inclined
plane (P). These weights resist the tendency of the coil to turn
round caused by the passage of a current through it, and serve
to bring the coil to its original position when the current
ceases. The cords by which these weights are suspended
pass through small holes in a piece of brass (*x*), whose dis-
tance from the coil can be varied by moving it up and down
along the vertical plane (P'), and thus the sensibility of the
apparatus can be altered. The strength of the field is so great
that the motion of the coil caused by the passage of a current
is almost non-oscillatory.

Attached to the coil (C) there is a thin circular disk of
ebonite (D), whose axis coincides with the vertical axis about
which the coil is free to turn, so that any angular motion of the
coil causes exactly the same angular motion of the disk. This
disk carries, on its underside and near to a portion of its
circumference, a number of platinum teeth, *t, t, t,* &c.
Directly underneath these teeth, and rigidly fixed to the
framework of the instrument, is a vessel (V), containing
acidulated water, and in this vessel is provided a capillary
arrangement which consists of two very narrow platinum
plates *p p* (fig. 3) (which shall, hereafter, be called capillary
plates), standing vertically up, side by side, from the central
part of the vessel, and drawing up the water of the vessel be-
tween them. The position of these capillary plates, when
everything is in its normal position, is such, that the platinum
tooth midway between the ends of the series *t* ... *t* is in con-
tact with the column of water between the capillary plates,
and that when the coil, and therefore the disk, is deflected to
the right or left, the other platinum teeth on the left or right

successively come into contact with the column of water between the capillary plates. Every time any one of the platinum teeth comes in contact with the water, it closes an electric circuit (to be described) corresponding to it, so that these platinum teeth may be called " circuit-closers."

(L) is a cylinder of wood lacquered all over. It is covered with platinum sheet, and on this sheet is rolled a ribbon of white paper nearly as wide as the length of the cylinder. A portion of this cylinder is in the rectangular box (B), which contains a chemical solution, consisting of ferrocyanide of potassium, nitrate of ammonium, and water mixed in a certain proportion. Further, the cylinder (L) is made to revolve with uniform velocity by means of a clockwork arrangement placed inside the box (H). Thus the paper on the cylinder, as it rotates, comes out moistened with the chemical solution. Resting on the cylinder (L), and fitting tightly in a rod of ebonite (*r*), there are a number of platinum needles *n, n, n,* &c.; these needles may be called "marking-needles," for, if an electric current passes between any of these platinum needles and the revolving paper, a bluish mark is made on the paper directly underneath that needle.

These marking-needles are electrically connected, each to each, with the circuit-closers in order, there being as many needles as there are circuit-closers; that is to say, the first needle (on the right or left) is in connection with the first circuit-closer (on the right or left), the second needle with the second circuit-closer, the third with the third, and so on. The small terminal screws, *a, a, a,* &c., on the ebonite plate (E), which is fixed to the framework of the apparatus, and also the screws, *b, b, b,* &c., are provided for facilitating these connections. The exceedingly fine wires (insulated) connect the screws *a, a, a,* &c., with the circuit-closers, and they all hang down from the screws in the form of spiral springs, meeting together in the common axis of the disk (D), and the coil (C), and thence go to the circuit-closers; so that it is to be understood that the resistance these wires offer to the motion of the disk and coil is so small as to be negligible.

Now, the platinum sheet on the cylinder (L) is in connection with one pole (Z) of the battery (CZ), by means of a platinum spring (*s*) resting on it; while the other pole (C) of the battery is in connection with the capillary plates (see fig. 3). Consequently, when there is no current passing through the coil, the positive current flows from the copper pole of the battery through the capillary plates, and the circuit-closer in the centre, and thence through the corresponding marking needle (the centre one), rotating paper, and platinum sheet,

and back to the zinc pole of the battery, making a blue mark on the rotating paper just underneath that needle ; while if a current passes through the coil it is deflected to the right or left according to the direction of the current, the circuit-closers on the left or right of the centre successively coming into contact with the column of water between the·capillary plates in order, the result being that the corresponding needles make blue marks on the rotating paper. But since the paper revolves with uniform velocity, it is evident that the longer the time of contact between a circuit-closer and the water between the capillary plates, no matter which circuit-closer it is, the longer the length of the mark on the paper underneath the needle corresponding to that circuit-closer ; and the shorter the shorter.

From the preceding description it will be clear that when an electric current, varying from time to time in strength and direction, passes through the coil (C), we shall get a curve made up of dots, or of dots and lines, on the moving paper ribbon, the nature of the curve determining the strength and direction of the current at any moment. Fig. 4 shows one of such curves experimentally obtained by allowing a varying current to pass through the coil. Now since the motion of the paper ribbon is uniform, it is easy to find out the point in the curve, or the position of the coil, corresponding to any moment; and since the motion of the coil is non-oscil-latory, each position of the coil corresponds to a certain definite strength of current, which can easily be determined by a simple experiment. So that by an examination of the curve thus obtained, it is easy to find out what was the strength of the current passing through the coil at any moment.

To give a rough idea of the sensibility of the apparatus, it may be mentioned that when the record shown in fig. 4 was obtained the apparatus was at its ordinary sensibility, which was such, that the superior and inferior limits of the current which it could record were respectively about 4 milliampères and $\frac{1}{6}$ of a milliampère. But of course the sensitiveness of the apparatus can be varied within a considerable range in very much the same way as in Thomson's Siphon Recorder.

One defect of the instrument, it may be argued, is the fact that it does not record any current which produces such a deflection of the coil that no one of the circuit-closers is in contact with the water between the capillary plates. This defect, however, is not a very serious one, for since the in-strument is intended to be used for recording varying currents which give rise to a curve made up of dots, or of dots and lines, on the moving paper ribbon, it is easy, by examining

the positions of dots and lines, to complete the curve to a certain degree of approximation. If, however, a greater accuracy be needed, all we have to do is to diminish the angular distance between the circuit-closers, and to increase their number. In the next instrument to be made, I am going to introduce a few improvements, of which the most important is the mode of arranging the circuit-closers and capillary plates. Instead of having the circuit-closers movable with the coil, and capillary plates fixed, we may arrange so that the capillary plates move with the coil, while the circuit-closers are kept stationary ; and by this means, it is possible to diminish the angular distances between the circuit-closers, and to increase their number without increasing the moment of inertia of the needle, and thus to obviate the above defect to a great extent, and, at the same time, to give to the instrument a greater sensibility.

XIII. *Electrochemical Researches.* By W. Ostwald, *Professor of Chemistry in the Polytechnic School, Riga**.

ALL reactions of acids, dependent on the characters, rather than on the nature of the constituents, of the acids, occur with an intensity which is different in each case, but is always proportional to an affinity-constant which is itself dependent only on the character of the acid and not at all on the nature of the reaction. This fundamental fact, which throws new light on the old conception of affinity-constants, has been proved by the author for various reactions ; viz. the formation of salts in aqueous solutions, the actions of acids on insoluble salts, the change of acetamide into ammonium acetate, the catalytic decomposition of methylic acetate, and the inversion of cane-sugar†. These reactions, some of which are statical and others kinetical, led to the same numerical values for the affinity-constants of the acids examined.

Adopting Clausius's theory of electrolysis, and Williamson's theory of chemical change, a distinct connection must exist between the reactions of acids and the electrical conductivity of these acids. The theory of Williamson supposes that a continual exchange of atoms is occurring among the reacting molecules ; the velocity of a chemical action must therefore depend on the velocity of the atomic interchanges. The theory of Clausius says that electrolytic conduction is effected so that the free ions continually displace equivalent elements or

* Abstract of Prof. Ostwald's recent work, prepared by himself, and communicated by M. M. Pattison Muir, Cambridge.
† See Pattison Muir's ' Principles of Chemistry,' p. 418 *et seq*.

radicles from the molecules, and that the parts of molecules set free again change places with others. The more frequently the ions can interchange the more rapidly will electricity be conducted, because the electricity can only pass along with the ions. Now as, according to Faraday's law, the ions always transmit the same quantity of electricity independently of their chemical character, it must be concluded from the theories of Clausius and Williamson that the velocities of reactions taking place under the influence of acids must be proportional to the velocities with which the acids transmit equal quantities of electricity, *i. e.* to the electrical conductivities of the acids*.

This inference from the theories of Williamson and Clausius has been verified by the author, by a series of measurements of the electrical conductivities of acids the velocities of reactions of which he had already determined ; the chief reactions in question were the catalytic decomposition of methylic acetate and the inversion of cane-sugar. The following Table (p. 106) presents the results. Columns I. and II. give the velocities of the reactions referred to that of hydrochloric acid as 100 ; the numbers hold good for solutions containing respectively $\frac{2}{3}$ and $\frac{1}{2}$ equivalents, in grammes, in 1000 cubic centimetres. Columns III., IV., and V. give the electrical conductivities, determined by the method of Kohlrausch, referred to that of hydrochloric acid as 100 ; the numbers in III. apply to normal solutions containing one gram-equivalent in 1000 cubic. centim. ; in IV., to $\frac{1}{10}$ normal ; and in V., to $\frac{1}{100}$ normal solutions.

These numbers show that the electrical conductivities of the acids in the table are proportional to the velocities of the change of methylic acetate into methylic alcohol and acetic acid, and the inversion of cane-sugar, brought about by these acids. The differences between the actual numbers in columns I., II., III., and columns IV. and V., may be explained by the occurrence of secondary actions among the first products (methyl alcohol, acetic acid, inverted sugar) of the two changes, measurements of the velocities of which were made. But determinations of the electrical conductivities of the acids are entirely free from the influences of secondary changes. By means of these determinations accurate values may be found for the affinity-constants of the acids. These values are as important in the theory of chemical affinity as the values of the equivalent weights of the elements are in Stoïchiometry.

* This conclusion has been already stated by Arrhenius (*Bigh. till. V. Svensk. Ak. Hand.* 8, Nos. 13 & 14, 1884) ; but it was based on a comparatively small number of experiments.

Acid.	Formula.	I.	II.	III.	IV.	V.
Hydrochloric......	HCl	100	100	100	118	124
Hydrobromic......	HBr	98	111	101	120	126
Nitric...............	NHO_3.............	92	100	99	117	123
Formic	HCO_2H	1·31	1·53	1·72	5·31	15·8
Acetic...............	$CH_3.CO_2H$	0·345	0·40	0·436	1·56	4·96
Chloracetic	$CH_2Cl.CO_2H$	4·300	4·84	5·06	15·3	38·9
Dichloracetic......	$CHCl_2.CO_2H$	23	27·1	24·7	64·2	100
Trichloracetic ...	$CCl_3.CO_2H$	68·2	75·4	61·1	100	110
Glycollic	$CH_2OH.CO_2H$....	......	1·31	1·39	4·65	13·9
Methoxyacetic ...	$CH_2OCH_3.CO_2H$...	......	1·82	1·79	6·61	19·2
Ethoxyacetic	$CH_2OC_2H_5.CO_2H$	......	1·37	......	5·46	16·5
Diglycollic........	$C(CH_2CO_2H)_2$	......	2·67	2·58	......	......
Propionic	$C_2H_5.CO_2H$........	0·304	......	0·325	......	......
Lactic...............	$C_2H_4OH.CO_2H$ ...	0·9	1·07	1·09	4·25	13·1
Oxypropionicβ ...	$C_2H_4OH.CO_2H$ ...	......	0·80	0·65	2·31	6·79
Glyceric............	$C_2H_3(OH)_2.CO_2H$	......	1·72	1·56	5·50	16·3
Pyruvic	$CH_3.CO.CO_2H$ ...	6·70	6·49	6·01	19·3	46·1
Butyric	$C_3H_7.CO_2H$	0·3	......	0·333	1·40	4·45
Isobutyric	$C_3H_7.CO_2H$	0·268	0·335	0·329	1·40	4·41
Oxyisobutyric ...	$C_3H_6OH.CO_2H$ ...	0·92	1·06	1·32	4·21	11·8
Oxalic...............	$(CO_2H)_2$...............	17·6	18·6	19·5	38·7	53·0
Malonic............	$CH_2(CO_2H)_2$........	2·87	3·08	3·16	9·52	24·4
Succinic	$C_2H_4(CO_2H)_2$	0·50	0·55	0·70	2·06	6·16
Malic	$C_2H_3OH(CO_2H)_2$ ...	1·18	1·27	1·40	4·79	13·9
Tartaric	$C_2H_2(OH)_2(CO_2H)_2$	2·30	......	2·37	6·89	20·9
Racemic...	$C_2H_2(OH)_2(CO_2H)_2$	2·30	......	2·37	6·89	20·9
Pyrotartaric	$C_3H_6(CO_2H)_2$	......	1·07	1·11	3·31	8·26
Citric	$C_3H_4OH(CO_2H)_3$ ...	1·63	1·73	1·73	5·49	14·3
Phosphoric ..,...	H_3PO_4	......	6·21	7·16	15·4	28·4
Arsenic	H_3AsO_4	......	4·81	5·32	12·4	25·5

In looking over the table we notice many relations between the affinity-constants and the chemical composition and constitution of the acids. Before, however, these relations can be inquired into it will be necessary to ask, What is the influence of dilution on the electrical conductivities of the acids? The numbers in columns III., IV., and V., which apply to solutions diluted in the ratio 1 : 10 : 100, show that the quantity of water present exerts a great and varying influence on the conductivities. It has been found by the author (*Journ. für prakt. Chemie*, [2] xxxi. p. 307, 1885) that the amount of water also influences the actions of acids in the inversion of cane-sugar, and that these actions vary in the same way as the electrical conductivites of the acids, for the various degrees of dilution.

The numbers in columns III., IV., and V. do not express the *specific conductivities* of the various acids, as this term is understood in Physics, but rather the products of the specific conductivities into the equivalent volumes of the solutions. The meaning here given to the term *equivalent-conductivity*

is as follows :—Let a vessel contain two parallel electrodes at a distance of 1 centim. apart, and let there be used one equivalent in grams of a specified acid; then the conductivity of the acid, when diluted with a definite quantity of water, is the equivalent-conductivity for that degree of dilution. It will afterwards be found more convenient to refer the conductivities of the acids to molecular rather than to equivalent weights. The *molecular conductivity* of an acid is found in a similar way to that whereby the equivalent-conductivity is determined. The degree of dilution is expressed by the number of litres of solution containing one molecular weight, in grams, of any specified acid. It would be more accurate to express the ratio of acid to water by a molecular ratio, *e. g.* $HCl : 100\,H_2O$; but, as only very dilute solutions are considered here, no essential inaccuracy will be introduced by adopting the more convenient method.

If the dilution $=0$ (that is, if no water is present), the conductivity of an acid is usually equal to, or is very little greater than, zero. As water is added the molecular conductivity increases, and approaches a maximum which is reached when the dilution is infinite. There is no exception to this general law. The conductivities of the stronger acids, $HCl, HBr, HI,$ $HNO_3, HClO_3, HClO_4, HBrO_3, HIO_3,$ &c., are nearly at their maxima in moderately dilute solutions ; the molecular conductivities of these acids vary but little with the dilution.

The following table presents the conductivities of some of the stronger acids when the dilution increases in the ratio of the powers of 2. The unit, in terms of which the conductivities are expressed, is $4 \cdot 248$ times greater than the mercury unit.

$v.$	HCl.	HBr.	HI.	$HNO_3.$	$HClO_3.$	$HClO_4.$
2	77·9	80·4	80·4	77·9	77·9	79·1
4	80·9	83·4	83·2	80·4	80·2	82·2
8	83·6	85·1	84·9	82·4	82·3	84·6
16	85·4	86·6	86·4	84·9	84·0	86·2
32	87·0	87·9	87·6	86·3	85·4	88·1
64	88·1	88·9	88·7	87·4	86·4	89·2
128	88·7	89·4	89·4	88·2	87·9	89·7
256	89·2	89·6	89·7	88·4	88·7	89·9
512	89·6	89·7	89·7	88·8	88·7	89·8
1024	89·5	89·5	89·3	88·9	88·6	89·8

The molecular conductivities of these acids are nearly the same (Kohlrausch had already observed this for some of these acids) ; they slowly increase and reach a maximum, equal to

about 90 for all these acids, at a dilution of 512 litres ; in more dilute solutions the conductivities are slightly diminished, owing to the impurities in the distilled water (*Journ. für prakt. Chemie*, [2] xxxi. p. 440). Whether the molecular conductivities of all acids reach a maximum equal to about 90 in very dilute solutions cannot be determined by direct experiments, because even with dilutions to 542 and 1024 litres the impurities present in the purest distilled water begin to exert an influence which cannot be accurately measured. An answer may, however, be given to this question if it is found possible to draw conclusions as to the behaviour of acids in more dilute solutions from their observed behaviour in less dilute solutions.

The observed values of the molecular conductivities of several acids are given in the following table. B = butyric, A = acetic, F = formic, M = monochloracetic, D = dichloracetic, H = hypophosphorous, I = iodic, acid; v = dilution, in litres.

v.	B.	A.	F.	M.	D.	H.	I.
2	0·394	0·520	1·758	4·994	25·72	30·89	42·57
4	0·604	0·755	2·465	6·984	34·32	37·91	50·56
8	0·876	1·078	2·431	9·531	43·00	45·81	59·0
16	1·278	1·514	4·796	12·86	52·17	54·13	66·3
32	1·810	2·123	6·634	17·29	60·25	62·10	72·3
64	2·560	2·943	9·180	22·85	67·40	69·06	76·9
128	3·594	4·084	12·59	29·61	72·45	74·05	80·2
256	5·036	5·642	16·98	37·81	76·24	77·84	81·8
512	7·015	7·753	22·43	46·75	79·76	79·92	83·0
1024	9·740	10·47	29·02	55·64	80·52	81·00	83·1
2048	13·37	14·44	35·83	63·48	80·87	81·39	82·9
4096	18·03	19·35	......	68·69	79·74	80·48	81·8

The molecular conductivity increases in every case, but in a very varying degree, with increasing dilution. The increase is greater, for a given increase of dilution, the smaller the conductivity of the acid ; it is also greater for weak than for strong acids, and greater for small than for large dilutions. The value of the increase in molecular conductivity for each dilution follows a special course : in the case of weak acids it increases as dilution increases ; the increase attains a maximum value, equal to 8·9, in the case of monochloracetic acid for a dilution of 512 litres (*mol. conduc.* at 256 litres = 37·8, and at 512 litres = 46·7, diff. = 8·9) ; the same value is attained for dichloracetic acid for the dilution from 8 to 16 litres (*mol. c.* = 43 and 52·1) ; approximately the same value is attained for hypophosphorous acid for the dilution from 8

to 16 litres (*mol. c.* = 45·8 and 54·1), and for iodic acid for the dilution from 4 to 8 litres (*mol. c.* = 50·5 and 59). As dilution increases in these cases, the increase in molecular conductivity for each dilution begins to decrease; for the strong acids the increase of conductivity is very small. It appears as if the maximum increase in molecular conductivity occurs where the conductivity is equal to 45—that is, is equal to half the maximum limit, 90. The relations between dilution and molecular conductivity are more definite in the various series of acids. The *mol. c.* of formic acid is 1·76 for the dilution 2 litres; almost the same value (1·81) is reached by butyric acid for the dilution 32 litres. The following table exhibits the numbers for these two acids :—

Formic Acid.	Dilution, in litres.		Butyric Acid.
1·76	2	32	1·81
2·47	4	64	2·56
3·43	8	128	3·59
4·80	16	256	5·04
6·33	32	512	7·02
9·18	64	1024	9·74
12·6	128	2048	13·4
17·0	256	4096	18·0
22·4	512	8192	23·8
29·0	1024	16384	31·5

The numbers run parallel to each other; those for butyric acid are about 5 per cent. greater than those for formic acid. Aqueous solutions of formic and butyric acids exhibit therefore about equal molecular conductivities when the latter is sixteen times more dilute than the former. If the data for the other acids are examined, it is apparent that *the dilutions at which the molecular conductivities of the monobasic acids exhibit equal values always bear a constant relation to each other*. This fundamental fact is exhibited by the following table, wherein equal molecular conductivities are placed in the same horizontal lines. The different series exhibit a closely corresponding course. The dilution is expressed by means of the exponent p, which is defined by the relation, dilution $= 2^p$. If the values of p and the molecular conductivities are regarded as coordinates of a curve, then the lines for the individual acids all form parts of one and the same curve which is common to all the acids. The point of origin for each acid must be specially chosen on the axis of the dilution-exponents p; in other words, a special constant must be given for the value of p for each acid.

Dil.	Butyric.	Dil.	Acetic.	Dil.	Formic.	Dil.	Monochloracetic.	Dil.	Dichloracetic.	Dil.	Hypophosphorous.	Dil.	Iodic.	Dil.	Chloric.
1	0·397	1	0·520	1	1·76	1	4·99	1	25·7	1	30·9	1	42·6	1	77·9
2	0·604	2	0·755	2	2·47	2	6·98	2	34·3	2	37·9	2	50·6	2	80·2
3	0·876	3	1·08	3	3·43	3	9·53	3	43·0	3	45·8	3	59·0	3	82·3
4	1·28	4	1·51	4	4·80	4	12·9	4	52·2	4	54·1	4	66·3	4	84·0
5	1·81	5	2·12	5	6·33	5	17·3	5	60·3	5	62·1	5	72·3	5	85·3
6	2·56	6	2·94	6	9·18	6	22·9	6	67·4	6	69·1	6	76·9	6	86·4
7	3·59	7	4·08	7	12·6	7	29·6	7	72·5	7	74·1	7	80·2	7	87·9
8	5·03	8	5·64	8	17·0	8	37·8	8	76·2	8	77·8	8	81·8	8	88·7
9	7·02	9	7·75	9	22·4	9	46·8					9	83·0		
10	9·74	10	10·47	10	29·0	10	55·6								

This result is identical with that already stated; for, if equal conductivities of the various acids are exhibited at equally proportional dilutions, then the dilution-exponents, which can be represented as logarithms of the dilutions (with the base 2), must show constant differences. The form of the curve is indicated by the results obtained; it runs asymptotic between the axis of p and a parallel placed at a distance equal to 90 units, and appears to be symmetric to right and left and also above and below. The maximum of increase, as already observed, lies at 45, where the curve shows an inflexion-point.
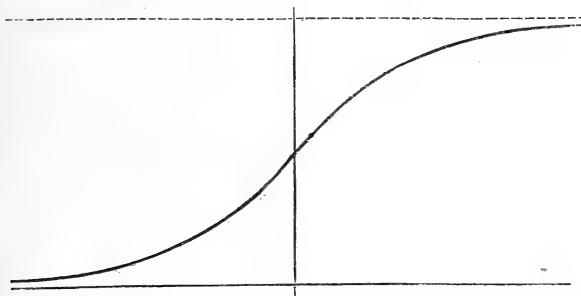
As we have here undoubtedly to do with a natural law, no exception to which is shown by any of the results obtained for 90 to 100 monobasic acids, it is reasonable to suppose that the curve must be capable of representation by a fairly simple analytical expression. At first sight a tangent-function is suggested. The results were therefore reduced so that the maximum fell at the value 90; they were then regarded as angles, and the corresponding tangents were found. The tangents, however, formed not an arithmetical but a geometrical series; the logarithms of the tangents gave approximately constant differences, they increased proportionately with the dilution-exponents. A few examples are given.

Acetic Acid.

p.	mol. c.	log tan mol. c.	Difference.
1	0·5196	7·9576	
2	0·7550	8·1199	0·1620
3	1·078	8·2745	0·1546
4	1·514	8·4220	0·1475
5	2·123	8·5690	0·1470
6	2·943	8·7110	0·1420
7	4·084	8·8537	0·1427
8	5·642	8·9947	0·1410
9	7·753	9·1340	0·1393
10	10·47	9·2667	0·1327

Formic Acid.

p.	mol. c.	log tan mol. c.	Difference.
1	1·758	8·4870	
2	2·465	8·6340	0·1470
3	3·431	8·7778	0·1438
4	4·796	8·9240	0·1462
5	6·634	9·0656	0·1416
6	9·180	9·2085	0·1429
7	12·59	9·3490	0·1405
8	16·98	9·4848	0·1358
9	22·43	9·6157	0·1309
10	29·02	9·7441	0·1284

Hypophosphorous Acid.

p.	mol. c.	log tan mol. c.	Difference.
1	30·89	9·7767	
2	37·91	9·8914	0·1147
3	45·81	0·0123	0·1209
4	54·13	0·1408	0·1285
5	62·10	0·2762	0·1354
6	69·06	0·4172	0·1410
7	74·05	0·5439	0·1267
8	77·84	0·6666	0·1227

The differences do not exhibit a constant value ; they are somewhat greater for the strong than for the weaker acids. The details are not given for the other acids, but only the mean values of the differences ; these mean values are as follows :—

Butyric... 0·155   Formic...... 0·140   Dichloracetic...... 0·133   Iodic... 0·126
Acetic ... 0·145   Chloracetic.. 0·136   Hypophosphorous 0·127   Chloric 0·140

The mean of these is 0·136 ; but the individual means deviate from this much more than can be accounted for by experimental errors. These deviations may be considered as follows :—If p_0 represent the dilution-exponent at which the conductivity is equal to half the maximum, 45 in the present units, then tan $45° =$ unity, and log tan $45° = 0$. Then for every other dilution-exponent p,

$$\log \tan m = ·136 \, (p - p_0) \; ;$$

where m is the molecular conductivity, referred to 90 as the maximum. Putting the quantity of water, v, in place of the dilution-exponent, p, we have $v = 2^p$, and $\log v = p \log 2 = ·30103p$, or $p = 3·032 \log v$, and the equation given above becomes

$$\log \tan m = ·136 \times 3·032 \, (\log v - \log v_0) = ·4124 \log \frac{v}{v_0},$$

whence

$$\tan m = \left(\frac{v}{v_0}\right)^{\cdot 4124}.$$

This final equation seems to be sufficiently simple. It includes one constant, v_0, dependent on the nature of the acid; the other constant, $\cdot 4124$, depends on the units chosen. But there seem to be two objections to the expression. There is a general agreement between the observed and calculated results, but individual acids show greater divergences than can be accounted for by experimental errors. If one attempts to explain these discrepancies, one is forced to admit that the formula has no rational foundation. An angle-function is never used in mathematical physics, so far as the author is aware, for a quantity which has no evident connection with angles.

The author has attempted to develop other, rationally grounded, expressions for the nature of the action of the water on the acid molecules; but none has yet been found to agree so closely with the observed results as that given above. The mathematical treatment of the problem must be reserved for future consideration.

The author then proceeds to discuss the results obtained with the polybasic acids. The behaviour of polybasic acids as regards dilution differs from that of monobasic acids. In one of his earlier papers the author suggested that when a solution of a polybasic acid is electrolyzed, only one of the hydrogen atoms of the acid goes to the cathode; the electrolysis takes place according to the scheme $H \mid HR''$ and $H \mid H_2R'''$. Sulphuric acid appeared as an exception. Further investigation has, however, shown that when the maximum conductivity is nearly reached by dilution, the second, and eventually the third, hydrogen atom takes part in the transfer of electricity, and electrolysis proceeds in accordance with the scheme $H_2 \mid R''$ and $H_3 \mid R'''$. The participation of the second and third atom of hydrogen depends upon the nature of the acids; those which have but feeble acid properties, *e. g.* selenious, phosphorous, or phosphoric acid (shown by the impossibility of titrating these acids by the use of litmus) exhibit molecular conductivities which follow much the same course as the conductivities of the monobasic acids. The results for some of the polybasic acids are given in the following tables; the molecular conductivities are referred to the same units as before. The values of log tan m and the differences are also given, so that comparisons may be made between the polybasic and monobasic acids.

Selenious Acid, H_2SeO_3.

p.	v.	m_1.	m_2.	m.	log tan m.	Diff.
1	2	7·636	7·642	7·639	9·1275	0·1070
2	4	9·752	9·720	9·736	9·2345	0·1184
3	8	12·73	12·66	12·70	9·3529	0·1215
4	16	16·57	16·62	16·60	9·4744	0·1261
5	32	21·75	21·73	21·73	9·6005	0·1296
6	64	28·25	28·23	28·24	9·7301	0·1336
7	128	36·23	36·15	36·15	9·8637	0·1380
8	256	45·21	45·00	45·11	0·0017	0·1414
9	512	54·44	54·10	54·27	0·1431	0·1458
10	1024	63·00	62·58	62·79	0·2889	
11	2048	69·80	69·00	69·40		
12	4096	73·98	73·18	73·58		

The behaviour of selenious acid is very similar to that of a monobasic acid, especially that of monochloracetic acid. If the numbers are divided by 2, the results represent the conductivities referred to an equivalent weight of selenious acid; the numbers thus obtained are not comparable with those tabulated in former series.

Phosphorous Acid, H_3PO_3.

p.	v.	m_1.	m_2.	m.	log tan m.	Diff.
1	2	28·63	28·62	28·63	9·7371	0·0966
2	4	34·30	34·28	34·29	9·9337	0·1076
3	8	41·17	41·11	41·14	9·9413	0·1209
4	16	49·17	49·00	49·09	0·0622	0·1246
5	32	56·96	56·96	56·96	0·1868	0·1351
6	64	64·65	64·39	64·52	0·3219	0·1220
7	128	70·28	70·14	70·21	0·4439	0·1143
8	256	74·69	74·39	74·54	0·5582	0·0986
9	512	77·84	77·30	77·57	0·6568	0·0598
10	1024	79·30	78·92	79·11	0·7158	
11	2048	80·00	79·50	79·75		
12	4096	79·60	78·54	79·07		

The differences are very small when the dilutions are large; this is probably due to formation of some phosphoric acid the molecular conductivity of which is smaller than that of phosphorous acid. In other respects the course of the change of conductivity of phosphorous acid is similar to that of the monobasic acids.

Those dibasic acids whose normal salts are not alkaline but neutral behave very differently. Even the weaker acids of this class show an increase of conductivity over the monobasic acids when the solutions become dilute; this advance of conductivity is exhibited sooner and to a greater extent the

stronger the acid. As examples, malonic and oxalic acids are given.

Malonic Acid, $CH_2(COOH)_2$.

p.	v.	m_1.	m_2.	m.	$\log \tan m$.	Diff.
1	2	4·48	4·48	4·48	8·8940	0·1497
2	4	6·32	6·35	6·34	9·0457	0·1476
3	8	8·90	8·83	8·87	9·1933	0·1397
4	16	12·16	12·14	12·15	9·3330	0·1406
5	32	16·60	16·54	16·57	9·4736	0·1387
6	64	22·39	22·15	22·27	9·6123	0·1393
7	128	29·45	29·43	29·44	9·7516	0·1371
8	256	37·95	37·53	37·74	9·8887	0·1433
9	512	47·40	46·82	47·11	0·0320	0·1454
10	1024	56·60	56·10	56·39	0·1774	0·1478
11	2048	65·18	64·20	64·69	0·3252	
12	4096	71·66	70·48	71·07		
13	8192	76·70	75·26	75·98		

At a dilution of 2 litres the conductivity of malonic acid is about 10 per cent. less than that of monochloracetic acid ; as dilution increases the difference between the conductivities of these two acids decreases, until the conductivities are equal at 512 litres dilution ; from this point onwards malonic acid surpasses acetic acid ; when the half maximum is past, the second hydrogen atom of malonic acid begins to take part in the electrolysis. Oxalic acid, which is a stronger acid than malonic, shows this greater conductivity than the monobasic

Oxalic Acid, $H_2C_2O_4$.

p.	v.	m_1.	m_2.	m.	$\log \tan m$.	Diff.
1	2	28·18	28·16	28·17	9·7288	0·1296
2	4	35·85	35·79	35·82	9·8584	0·1283
3	8	44·14	44·10	44·12	9·9867	0·1330
4	16	52·7	52·8	52·8	0·1197	0·1437
5	32	61·3	61·4	61·4	0·2634	0·1524
6	64	69·0	69·0	69·0	0·4158	0·1592
7	128	75·1	75·0	75·1	0·5750	0·1699
8	256	79·8	79·8	79·8	0·7449	0·2052
9	512	83·6	83·5	83·6	0·9501	0·3763
10	1024	87·2	87·3	87·3	1·3264	
11	2648	92·1	91·9	92·0		
12	4096	98·9	98·4	98·7		
13	8192	118·5	118·5	118·5		
14	16384					

acids in a very marked way ; the maximum of the monobasic acids is passed by oxalic acid. The falling off in the conductivity, which is customary with the monobasic acids when the dilution is greater than 1024 litres, is hidden by the partici-

pation in the electrolysis of the second hydrogen atom of oxalic acid. The marked increase in the value of log tan m is very characteristic.

Sulphuric acid is stronger than oxalic acid; the second hydrogen atom of sulphuric acid will therefore probably sooner take part in the electrolysis, the maximum of the monobasic acids will be overstepped, and another maximum will be approached which we may suppose will be double as large as that of the monobasic acids. The following numbers were obtained :—

Sulphuric Acid, H_2SO_4.

p.	v.	m_1.	m_2.	m.
1	2	92·7	92·7	92·7
2	4	96·4	96·4	96·4
3	8	100·7	100·5	100·6
4	16	107·5	107·2	107·4
5	32	116·3	116·2	116·3
6	64	127·0	127·4	127·2
7	128	139·5	138·9	139·2
8	256	150·6	150·6	150·6
9	512	161·0	160·8	160·9
10	1024	169·3	168·9	169·1
11	2048	174·5	174·3	174·4
12	4096	177·1	177·0	177·1
13	8192	177·1	176·6	176·9
14	16384	174·3	174·1	174·2

Neither these numbers, nor those obtained by referring the conductivities to the equivalent weight of sulphuric acid, can be made to agree with the normal curve. The separate actions of the two hydrogen atoms of sulphuric acid are shown very markedly in the results recently obtained by F. Kohlrausch (*Gött. Nachr.* 1885, p. 80). The conductivities of the stronger monobasic acids appear as straight lines in the system of coordinates chosen by Kohlrausch ; the curve of sulphuric acid forms two straight lines at different inclinations, joined by a short curve which falls at the dilution 2–8 litres. The table given by the author contains therefore the second part of the complete curve. As Kohlrausch did not examine any other dibasic acid of nearly the same strength as sulphuric, the behaviour of this acid remains unexplained by his results.

The point at which the second hydrogen atom of a very strong dibasic acid begins to take part in the electrolysis is situated in the concentrated solution. Dilute solutions of such an acid behave similarly with the monobasic acids, provided that molecule is not compared with molecule, but

equivalent with equivalent. Methylene-disulphonic acid, $CH_2 (SO_2OH)_2$, is a very strong dibasic acid; solutions of this acid behave similarly to those of nitric acid.

Methylene-disulphonic Acid, $CH_2 (SO_2OH)_2$.

$p.$	$v.$	$m_1.$	$m_2.$	$m.$	Equiv.
1	2	133·7	135·1	134·4	67·2
2	4	146·0	146·7	146·4	73·2
3	8	153·0	153·6	153·3	76·7
4	16	158·9	158·5	158·7	79·4
5	32	163·5	164·0	163·3	81·9
6	64	167·7	167·3	167·5	83·8
7	128	171·4	171·0	171·2	85·6
8	256	175·0	173·3	174·2	87·1
9	512	177·8	175·9	176·9	88·5
10	1024	178·8	177·2	178·0	89·0
11	2048	179·3	178·5	178·9	89·5
12	4096	179·2	177·2	178·2	89·1
13	8192	177·1	175·7	176·4	88·2

The last column, headed Equiv., shows that the conductivity referred to the equivalent weight (half the molecular weight) of this acid varies in the same way as the conductivity of a strong monobasic acid. The limiting value of the molecular conductivity of dibasic acids, when the second phase of electrolysis is traversed, is shown by the numbers in the above table to be twice as large as that for monobasic acids.

The relations between dilution and conductivity of tribasic and polybasic acids are analogous to those already discussed. Phosphoric acid reacts with two equivalents of alkali to form nearly neutral salts; the third hydrogen atom is that of a very weak acid. In conformity with these facts, we find the conductivity only slightly increased by great dilution.

Phosphoric Acid, H_3PO_4.

$p.$	$v.$	$m_1.$	$m_2.$	$m.$	$\log \tan m.$	Diff.
1	2	14·21	14·23	14·22	9·4038	0·0815
2	4	16·99	17·01	17·00	9·4853	0·1047
3	8	21·22	21·30	21·26	9·5900	0·1189
4	16	27·05	27·12	27·09	9·7089	0·1268
5	32	34·41	34·41	34·41	9·8357	0·1347
6	64	42·85	43 24	43 05	9·9704	0·1541
7	128	53·1	53·0	53·1	0·1245	0·1462
8	256	61·8	61·8	61·8	0·2707	0·1659
9	512	69·9	69·6	69·9	0·4366	0·1476
10	1024	75·4	75·4	75·4	0·5842	
11	2048	79·0	78·9	79·0		
12	4096	79·8	79·8	79·8		
13	8192	78·9	78·7	78·8		

Phosphoric acid closely resembles dichloracetic acid ; only in very dilute solutions does it surpass that acid.   Citric acid corresponds with malonic acid in behaviour on dilution ; it forms no salts with alkaline reaction, but is much weaker than phosphoric acid.   Strong tribasic acids have not yet been examined ; but their behaviour may be deduced from the results already obtained.

The results for dibasic and tribasic acids cannot be brought into mathematical form until a rational expression is found for the law of dilution of the monobasic acids.   It is evident that the dibasic acids must be regarded as to some extent the sums of two monobasic acids having different conductivities. The prospect presents itself of finding numerical expressions for the function of the replaceable hydrogen of the polybasic acids.

XIV. *On the Self-induction of Wires.* *By* OLIVER HEAVISIDE[*].

A SERIES of experiments made some years ago, in which I used the Wheatstone-bridge and the differential telephone as balances of induction as well as of resistance, led me to undertake a theoretical investigation of the phenomena occurring when conducting-cores are placed in long solenoidal coils, in which impressed electromotive force is made to act, in order to explain the disturbances of balance which are produced by the dissipation of energy in the cores.   The simpler portions of this investigation, leaving out those of greater mathematical difficulty and less practical interest, relating to hollow cores and the effect of allowing dielectric displacement, were published in the 'Electrician' from May 3, 1884, to January 3, 1885.

This investigation led me to the mathematically similar investigation of the transmission of current into wires.   I say *into* wires, instead of through wires, because the current is really transmitted by diffusion from the boundary into a wire from the external dielectric, under all ordinarily occurring circumstances.   In the case of a core placed in a coil the magnetic force is longitudinal and the current circular ; in the case of a straight round wire the current is longitudinal and the magnetic force circular. The transmission of the longitudinal current into the wire takes place, however, exactly in the same manner as the transmission of the longitudinal magnetic force into the core within the coil, when the boun-

dary conditions are made similar, which is easily to be realized. Similarly we may compare the circular electric current in the core to the circular magnetic current in the wire.

I also found the transfer of energy to be similar in both cases, viz. radially inward or outward to or from the axis of the core or the wire. It was therefore necessary to consider the dielectric, in order to complete the course of the transfer of energy from its source, say a voltaic cell, to its sink, the wire or the core where it is finally dissipated in the form of heat, and its temporary storage as electric and magnetic energy in the field generally, including the conductors.

Terminating the paper above referred to, having so much other matter, I started a fresh one under the title of " Electromagnetic Induction and its Propagation," commencing in the ' Electrician,' January 3, 1885. Having, according to my sketched plan, to get rid of general matter first, before proceeding to special solutions, I took occasion near the commencement of the paper to give a general account of some of my results regarding the propagation of current, in which the following occurs, describing the way the current rises in a wire, and the consequent approximation, under certain circumstances, to mere surface-conduction. It was meant to illustrate the previously-mentioned stoppage of current-conduction by high conductivity. After an account of the transfer of energy through the dielectric (concerning which I shall say a few words later) I continue (' Electrician,' January 10, 1885):—

" Since, on starting a current, the energy reaches the wire from the medium without, it may be expected that the electric current is first set up in the outer part, and takes time to penetrate to the middle. This I have verified by investigating some special cases.

" Increase the conductivity enormously, still keeping it finite, however. Let it, for instance, take minutes to set up a current at the axis. Then ordinary rapid signalling 'through the wire' would be accompanied by a surface-current only, penetrating to but a small depth. The disturbance is thus propagated parallel to the wire in the manner of waves, with reflection at the end, and hardly any tailing off. With infinite conductivity there can be no current set up in the wire at all. There is no dissipativity ; wave-propagation is perfect. The wire-current is wholly superficial, an abstraction, yet it is nearly the same with very high conductivity. This illustrates the impenetrability of a perfect conductor to magnetic induction (and similarly to electric currents) applied by Maxwell to the molecular theory of magnetism." . . . . .

Attention has recently been forcibly directed towards the phenomenon above described of the inward transmission of current into wires by Professor Hughes's Inaugural Address to the Society of Telegraph Engineers and Electricians, January 1886. This paper was, for many reasons, perhaps the most remarkable one ever written. It was remarkable for the extraordinary ignoration of well-known facts, thoroughly worked out already ; it was remarkable for a quite phenomenal mixing up of the effects due to induction and to resistance, and the author's apparent inability to separate them, or to see the real meaning of his results ; one might indeed imagine that an entirely new science of induction was in its earliest stages. It was remarkable that the great experimental skill of the author should have led him to employ a method which was in itself highly objectionable, being capable of giving, in general, neither a true resistance nor a true induction-balance (as may be very easily seen by simple experiments with coils, without any mathematical examination of the theory)—a method which does not therefore admit of any exact interpretation of results without the fullest particulars being given and subjected to laborious calculations ; and finally, it was remarkable as containing, so far as could be safely guessed at, many verifications of the approximation towards mere surface-conduction in wires. This is, after all, the really important matter, against which all the rest is insignificant.

As regards the method employed, I have shown its inaccuracy in a paper " On the Use of the Bridge as an Induction-balance," in ' The Electrician,' April 30, 1886, wherein I also described correct methods, including the simple bridge without mutual induction, and also methods in which mutual induction is employed to get balance, giving the requisite formulæ, which are of the simplest character.

As regards the interpretation of Prof. Hughes's thick-wire results, showing departure from the linear theory, by which I mean the theory that ignores differences in the current-density in wires, I made the following remarks in the ' Electrician,' April 23, 1886. After commenting upon the difficulty of exact interpretation, I proceed :—

" The most interesting of the experiments are those relating to the effect of increased diameter on what Prof. Hughes terms the inductive capacity of wires. My own interpretation is roughly this. That the time-constant of a wire first increases with the diameter " [this is of course what the linear theory shows], " and then, later, decreases rapidly ; and that the decrease sets in the sooner the higher the conductivity and the higher the inductivity (or magnetic permeability) of the

wires. If this be correct, it is exactly what I should have
expected and predicted. In fact, I have already described
the phenomenon in this Journal ; or, rather, the phenomenon
I described contains in itself the above interpretation. In
the 'Electrician' for January 12, 1884" [corrected to
January 10, 1885, in a subsequent letter, May 7, 1886], "I
described how the current starts in a wire. It begins on its
boundary, and is propagated inward. Thus during the rise
of the current it is less strong at the centre than at the boun-
dary. As regards the manner of inward propagation, it takes
place according to the same laws as the propagation of mag-
netic force and current into cores from an enveloping coil,
which I have described in considerable detail in this Journal.
The retardation depends upon the conductivity, upon the
inductivity, and upon the section, under similar boundary-
conditions. If the conductivity be high enough, or the induc-
tivity, or the section be large enough, to make the central
current appreciably less than the boundary-current during the
greater part of the time of rise of the current, there will be
an apparent reduction in the time-constant. Go to an ex-
treme case—very rapid short currents, and large retardation
to inward transmission. Here we have the current in layers,
strong on the boundary, weak in the middle. Clearly then,
if we wish to regard the wire as a mere linear circuit, which
it is not, and as we can only do to a first approximation, we
should remove the central part of the wire—that is, increase
its resistance, regarded as a line, or reduce its time-constant.
This will happen the sooner the greater the inductivity and
the conductivity, as the section is continuously increased. It
is only thin wires that can be treated as mere lines, and even
they, if the speed be only great enough, must be treated as
solid conductors. I ought also to mention that the influence
of external conductors, as of the return conductor, is of im-
portance, sometimes of very great importance, in modifying
the distribution of current in the transient state. I have had
for years in manuscript some solutions relating to round wires,
and hope some day to arrive at them in the course of the
paper I am at present publishing, or, rather, not publishing, as
the editor has been able to afford so little space for it lately.

" As a general assistance to those who go by old methods,
a rising current inducing an opposite current in itself and in
parallel conductors, this may be useful. Parallel currents are
said to attract or repel, according as the currents are together
or opposed. This is, however, mechanical force on the con-
ductors. The distribution of current is not affected by it.
But when currents are increasing or decreasing, there is an

apparent attraction or repulsion between them. Oppositely-
going currents repel when they are decreasing and attract
when they are increasing.   Thus, send a current into a loop,
one wire the return to the other, both being close together.
During the rise of the current it will be denser on the sides
of the wires nearest one another than on the remote sides. . . . ."

An iron wire, through which rapid reversals are sent, should
afterwards be found, by reason of its magnetic retentiveness,
magnetized in concentric cylindrical shells, of alternately
positive and negative magnetization.   This would only occur
superficially.   The thickness of the layers would give infor-
mation regarding the amount of retardation, from which the
inductivity could be deduced.   The case is similar to that of
the superficial layers of magnetization produced in a core in
a coil through which reversals are sent, the magnetization
being then, however, longitudinal instead of circular.

The linear theory is departed from in the most extreme
manner, when the return-current closely envelops the wire.
The theory of the rise of the current in this case I have given
in the 'Electrician' for May 14, 1886, and the case of the
return-current at any distance is considered June 11, and
will be followed by others.   The investigation following in
this paper is more comprehensive, taking into account both
electrostatic and electromagnetic induction, working down to
the electromagnetic theory on the one hand, and approxima-
ting towards the electrostatic theory (long submarine cable)
on the other ; with this difference, that inertia is not so
wholly ignorable in the long-line case as is elastic yielding in
the case of a short wire.   Nor is the variation of current-
density wholly ignorable.

But first as regards the transfer of energy in the electro-
magnetic field.   This is a very important matter theoretically.
It is a necessity of a rationally intelligible scheme (even if it
be only on paper) that the transfer of energy should be expli-
citly definable.   It is the absence of this definiteness that
makes the German methods so repulsive to a plain man who
likes to see where he is going and what he is doing, and hates
metaphysics in science.

I found that I had been anticipated by Prof. Poynting in
the deduction of the transfer of energy formula appropriate
to Maxwell's electromagnetic scheme, in the main.   It is,
therefore, only as having given the equation of activity in a
more general form, the most general that Maxwell's scheme
admits of, and having deduced it in a simple manner, that I
can attach myself to the matter.   In connection with it, how-
ever, there is another matter of some importance, viz. the use

of a certain fundamental equation. That I should have been
able to arrive at the most general form, taking into account
intrinsic magnetization, as well as not confining myself to
media homogeneous and isotropic as regards the three quan-
tities conductivity, inductivity, and dielectric capacity, in a
simple and direct manner, without any volume-integrations or
complications, arose from my method of treating the general
equations. I here sketch out the scheme, in the form I give it.

Let $\mathbf{H}_1$ be the magnetic force and $\boldsymbol{\Gamma}$ the current. (Thick
letters here for vectors. The later investigation is wholly
scalar.) Then, "curl" denoting the well-known rotatory
operator, Maxwell's fundamental current equation is

$$\operatorname{curl} \mathbf{H}_1 = 4\pi\boldsymbol{\Gamma}, \quad \ldots \quad \ldots \quad (1)$$

and is his definition of electric current in terms of magnetic
force. It necessitates closure of the electric current, and, at
a surface, tangential continuity of $\mathbf{H}_1$ and normal continuity of
$\boldsymbol{\Gamma}$. The electric current may be conductive, or the variation
of the elastic "displacement," say

$$\boldsymbol{\Gamma} = \mathbf{C} + \dot{\mathbf{D}}.$$

$\mathbf{C}$ being the conduction-current, $\mathbf{D}$ the displacement, linear
functions of the electric force $\mathbf{E}$, thus

$$\mathbf{C} = k\mathbf{E}_1, \quad \mathbf{D} = c\mathbf{E}_1/4\pi \;;$$

k being the conductivity, and c the dielectric capacity (or $c/4\pi$
the condenser-capacity per unit volume). Equation (1) thus
connects the electric and the magnetic forces one way. But
this is not enough to make a complete system. A second
relation between $\mathbf{E}_1$ and $\mathbf{H}_1$ is wanted.

Maxwell's second relation is his equation of electric force
in terms of two highly artificial quantities, a vector and a
scalar potential, say $\mathbf{A}$ and P, thus

$$\mathbf{E}_1 = -\dot{\mathbf{A}} - \nabla\mathrm{P}, \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

ignoring impressed force for the present. From $\mathbf{A}$ we get
down to $\mathbf{H}_1$ again, thus,

$$\operatorname{curl} \mathbf{A} = \mathbf{B},$$

$$\mathbf{B} = \mu\mathbf{H}_1 \;;$$

$\mathbf{B}$ being the magnetic induction, and μ the inductivity. (Here
we ignore intrinsic magnetization.)

The equation (2) is arrived at through a rather complex
investigation. From these equations are deduced the general
equations of electromagnetic disturbances in vol. ii. art. 783.
They contain both $\mathbf{A}$ and P. One or other of them must go
before we can practically work them, which are, independently

of this, rather unmanageable, although they are not really general, for impressed forces are omitted, and the intrinsic magnetization must be zero, and the medium isotropic. Again, and this is an objection of some magnitude, the two potentials **A** and P, if given everywhere, are *not sufficient* to specify the state of the electromagnetic field. Try it; and fail.

Even without using these complex general equations referred to, but those on which they are based, (1) and (2), the very artificial nature of **A** and P greatly obscures and complicates many investigations. Not being able to work practically in terms of **A** and P in a general manner, and yet knowing there was nothing absolutely wrong, I went to the root of the evil, and cured it, thus:—

As a companion to equation (1) use this,

$$-\operatorname{curl} \mathbf{E}_1 = 4\pi\mathbf{G}; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where **G** is the magnetic current, or $\dot{\mathbf{B}}/4\pi$. That this may be derived at once from (2) is obvious. But what is of greater importance in view of the difficult establishment of (2), is that (3) can be got immediately independently, and that (2) is its consequence. Equation (3) is in fact the mathematical expression of the Faraday law of induction, that the electromotive force of induction in any closed circuit is to be measured by the rate of decrease of the induction through it.

Now make (1) and (3) the fundamental equations of motion, and ignore (2) altogether, except for special purposes. There are several great advantages in the use of (3). First, the abolition of the two potentials. Next, we are brought into immediate contact with $\mathbf{E}_1$ and $\mathbf{H}_1$, which have physical significance in really defining the state of the medium anywhere (k, μ, and c of course to be known), which **A** and P do not, and cannot, even if given over all space. Thirdly, by reason of the close parallelism between (1) and (3), electric force related to magnetic current, as magnetic force to electric current, we are enabled to easily perceive many important relations which are not at all obvious when the potentials **A** and P are used, and (3) ignored. Fourthly, we are enabled with considerable ease, if we have obtained solutions relating to variable states in which the lines of $\mathbf{E}_1$ and $\mathbf{H}_1$ are related in one way, to at once get the solutions of problems of quite different physical meaning, in which $\mathbf{E}_1$ and $\mathbf{H}_1$, or quantities directly related to them, change places. For example, the variation of magnetic force in a core in a coil, and of electric current in a round wire; and many others.

That the advantages attending the use of (3) as a fundamental equation are not imaginary, I have repeatedly verified.

The establishment of the general equation of activity, however, which I now give (' Electrician,' February 21, 1885), shows that (3) is really the proper and natural fundamental equation to use. But we must first introduce impressed forces, allowing energy to be taken in by the electric and magnetic currents. In (1) and (3) $\mathbf{E}_1$ and $\mathbf{H}_1$ are not the effective electric and magnetic forces concerned in producing the fluxes conduction-current, displacement, and induction, but require impressed forces, say $\mathbf{e}$ and $\mathbf{h}$, to be added. Let $\mathbf{E}=\mathbf{E}_1+\mathbf{e}$, &c.; then we shall have

$$\mathbf{B}=\mu\mathbf{H}, \quad \mathbf{C}=k\mathbf{E}, \quad \mathbf{D}=c\mathbf{E}/4\pi, \quad . \quad . \quad . \quad (4)$$

as the three linear relations between forces and fluxes; two equations,

$$\mathbf{\Gamma}=\mathbf{C}+\dot{\mathbf{D}}, \quad \mathbf{G}=\dot{\mathbf{B}}/4\pi, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

showing the structure of the currents; and two equations of cross-connection,

$$\text{curl } (\mathbf{H}-\mathbf{h})=4\pi\mathbf{\Gamma}, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$-\text{curl } (\mathbf{E}-\mathbf{e})=4\pi\mathbf{G}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Next, let Q be the dissipativity, U the electric energy, and T the magnetic energy per unit volume, defined thus :

$$Q=\mathbf{EC}, \quad U=\tfrac{1}{2}\mathbf{ED}, \quad T=\tfrac{1}{2}\mathbf{HB}/4\pi \quad . \quad . \quad .. \quad (8)$$

(according to the notation of scalar products used in my paper in the Philosophical Magazine, June 1885 ; c, k, and μ are in general the operators appropriate to linear connection between forces and fluxes). Then we get the full equation of activity at once, by multiplying (6) by $\mathbf{E}$ and (7) by $\mathbf{H}$ and adding the results. It is

$$\left.\begin{aligned} \mathbf{e}\mathbf{\Gamma}+\mathbf{h}\mathbf{G}&=\mathbf{E}\mathbf{\Gamma}+\mathbf{H}\mathbf{G}+ \text{div. } \mathbf{V}(\mathbf{E}-\mathbf{e})(\mathbf{H}-\mathbf{h})/4\pi, \\ &=Q+\dot{U}+\dot{T} + \text{div. } \mathbf{V}(\mathbf{E}-\mathbf{e})(\mathbf{H}-\mathbf{h})/4\pi, \end{aligned}\right\} \quad (9)$$

where div. stands for divergence, the negative of Maxwell's convergence. The left side showing the energy taken in per second per unit volume by reason of impressed forces, and $Q+\dot{U}+\dot{T}$ being expended on the spot in heating, and increasing the electric and magnetic energies, we see that $\mathbf{V}(\mathbf{E}-\mathbf{e})(\mathbf{H}-\mathbf{h})/4\pi$ is the vector transfer of energy per unit area per second, or the energy-current density. The appropriateness of (7) as a companion to (6) is very clearly shown.

The scheme expressed by (4), (5), (6), (7) is, however, in one respect too general. The magnetic current is closed, by

(7) ; but that does not necessitate the closure of the magnetic induction, which is necessary to avoid having unipolar magnets. Hence

$$\text{div. } \mathbf{B} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

is required to meet facts, in addition to (4), (5), (6), (7). There is no magnetic conduction-current with dissipation of energy, analogous to the electric conduction-current.

As regards the meanings of **e** and **h**, in the light of dynamics they define themselves in the equation of activity ; that is, so far as the mere measure of impressed forces is concerned, apart from physical causation. Thus **e** is the amount of energy taken in by the electromagnetic field per second per unit volume per unit electric current, and **h** is similarly related to magnetic current. Under **e** has to be included the recognized voltaic and thermoelectric forces. But besides them, it has to include the impressed electric force due to motion in a magnetic field, or $\mathbf{VvB}$ if **v** is the vector velocity, necessitating a mechanical force $\mathbf{VFB}$. It has also to include intrinsic electrization, the state which is set up in solid dielectrics under the continued application of electric force. Thus
$$\mathbf{J} = c e / 4\pi$$

connects the intensity of intrinsic electrization **J** with the corresponding **e**.

I can only find two kinds of **h**. First, due to motion in an electric field, viz. $4\pi\mathbf{VDv}$, necessitating a mechanical force $4\pi\mathbf{VDG}$; and, secondly, much more importantly, intrinsic magnetization **I**, connected with the corresponding **h** thus,

$$\mathbf{I} = \mu\mathbf{h}/4\pi.$$

As regards potentials, there are, to match the two electric potentials **A** and P, two magnetic potentials, say **Z** and Ω ; Ω being the single-valued scalar magnetic potential, and **Z** the vector potential of the magnetic current, some of whose properties in relation to dielectric and conductive displacement I have worked out in the paper referred to before.

As regards the general equations of disturbances, like Maxwell's (7), chapter xx. vol. ii., they are far more a hindrance than an assistance in general investigations. But when we come to a special investigation, and need to know the forms of the functions involved, then we may eliminate either **E** or **H** between (6) and (7), and use the suitable coordinates.

We may make use of the above equations at the start, in passing to the question of the propagation of disturbances along a wire, after which the investigation will be wholly scalar. Put $\mathbf{e} = 0$ in (7) ; then we see that we cannot alter

the magnetic force at a point without giving rotation to the electric force. Now as in a steady state the electric force has no rotation (away from the seat of impressed force), it follows that under no circumstances (except by artificial arrangements of impressed force) can we set up the steady state in a conductor strictly according to the linear theory. We may approximate to it very closely throughout the greater part of the variable period, but it will be widely departed from in the very early stages.

Let there be a straight round wire of radius a_1, conductivity k_1, inductivity μ_1, and dielectric capacity c_1; surrounded up to radius a_2 by a dielectric of conductivity k_2, inductivity μ_2, and dielectric capacity c_2: in its turn surrounded to radius a_3 by a conductor of k_3, μ_3, and c_3. This might be carried on to any extent; but we stop at $r = a_3$, r being distance from the axis of the wire, as the outer conductor is to be the return to the inner wire.

Let the magnetic lines be such as would be produced by longitudinal impressed electric force, viz. circles in planes perpendicular to the axis of the wire and centered thereon. Let H be the intensity of magnetic force at distance r from the axis, and distance z along it from a fixed point. Use (6), with $\mathbf{h} = 0$, to find the electric current. It has two components, say Γ longitudinal or parallel to z, and γ radial, or parallel to r, given by

$$4\pi\Gamma = \frac{1}{r}\frac{d}{dr}r\mathrm{H}, \quad 4\pi\gamma = -\frac{d\mathrm{H}}{dz}. \quad . \quad . \quad . \quad (11)$$

We have also $\mathbf{E} = \rho\boldsymbol{\Gamma}$, if ρ is a generalized resistivity, or

$$\rho^{-1} = k + \frac{c}{4\pi}\frac{d}{dt}. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Now use equation (7), with $\mathbf{e} = 0$. The curl of the longitudinal and of the radial electric force are both circular, like H, giving

$$\mu\dot{\mathrm{H}} = \rho\left(\frac{d\Gamma}{dr} - \frac{d\boldsymbol{\gamma}}{dz}\right). \quad . \quad . \quad . \quad . \quad (13)$$

In this use (11), and we get the H equation, which is

$$\frac{d}{dr}\frac{1}{r}\frac{d}{dr}r\mathrm{H} + \frac{d^2\mathrm{H}}{dz^2} = 4\pi\mu k\dot{\mathrm{H}} + \mu c\ddot{\mathrm{H}}. \quad . \quad . \quad (14)$$

The suffixes $_1$, $_2$, and $_3$ to be used, according as the wire, dielectric or sheath, is in question.

In a normal state of free subsidence, $d/dt = p$ a constant. Let also $d^2/dz^2 = -m^2$, where m^2 is a constant, depending upon

the terminal conditions. Also, let

$$-s^2 = 4\pi\mu k p + \mu c p^2 + m^2. \quad . \quad . \quad . \quad (15)$$

Then (14) becomes

$$\frac{d}{dr}\frac{1}{r}\frac{d}{dr}rH + s^2H = 0; \quad . \quad . \quad . \quad (16)$$

which is the equation of the $J_1(sr)$ and its complementary function, which call $K_1(sr)$. Thus, for reference,

$$\left.\begin{array}{l}
J_0(sr) = 1 - \dfrac{s^2r^2}{2^2} + \dfrac{s^4r^4}{2^24^2} - \cdots, \\[2mm]
J_1(sr) = -\dfrac{d}{d(sr)}J_0(sr) = \tfrac{1}{2}sr\left(1 - \tfrac{1}{2}\dfrac{s^2r^2}{2^2} + \tfrac{1}{3}\dfrac{s^4r^4}{2^24^2} - \cdots\right), \\[2mm]
K_0(sr) = J_0(sr).\log sr + \dfrac{s^2r^2}{2^2} - (1 + \tfrac{1}{2})\dfrac{s^4r^4}{2^24^2} + \cdots, \\[2mm]
K_1(sr) = -\dfrac{d}{d(sr)}K_0(sr) = -\dfrac{J_0(sr)}{sr} + J_1(sr)\log sr \\[2mm]
\qquad\qquad\qquad - \tfrac{1}{2}sr\left\{1 - \tfrac{3}{4}\dfrac{s^2r^2}{2^24^2} + \cdots\right\}.
\end{array}\right\} \quad (17)$$

We have therefore the following sets of solutions, in the wire, dielectric, and sheath respectively, the A's and B's being constants :—

$$\left.\begin{array}{l}
H_1 = A_1J_1(s_1r)\cos(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\gamma_1 = A_1J_1(s_1r)m\sin(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\Gamma_1 = A_1J_0(s_1r)s_1\cos(mz+\theta)\epsilon^{pt}, \\[1mm]
H_2 = \{A_2J_1(s_2r) + B_2K_1(s_2r)\}\cos(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\gamma_2 = \{A_2J_1(s_2r) + B_2K_1(s_2r)\}m\sin(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\Gamma_2 = \{A_2J_0(s_2r) + B_2K_0(s_2r)\}s_2\cos(mz+\theta)\epsilon^{pt}, \\[1mm]
H_3 = \{A_3J_1(s_3r) + B_3K_1(s_3r)\}\cos(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\gamma_3 = \{A_3J_1(s_3r) + B_3K_1(s_3r)\}m\sin(mz+\theta)\epsilon^{pt}, \\[1mm]
4\pi\Gamma_3 = \{A_3J_0(s_3r) + B_3K_0(s_3r)\}s_3\cos(mz+\theta)\epsilon^{pt}.
\end{array}\right\} \quad . \quad (18)$$

To harmonize these, we have the boundary conditions of continuity of tangential electric and magnetic forces, and of normal electric and magnetic currents (or of magnetic induction). Thus $\gamma_1 = \gamma_2$ and $\rho_1\Gamma_1 = \rho_2\Gamma_2$, at $r = a_1$, give us

$$\left.\begin{array}{l}
A_2/A_1(J_1K_0 - J_0K_1)(s_2a_1) = J_1(s_1a_1)K_0(s_2a_1) \\[1mm]
\qquad\qquad\qquad - (\rho_1s_1/\rho_2s_2)J_0(s_1a_1)K_1(s_2a_1), \\[1mm]
B_2/A_1(J_1K_0 - J_0K_1)(s_2a_1) = (\rho_1s_1/\rho_2s_2)J_0(s_1a_1)J_1(s_2a_1) \\[1mm]
\qquad\qquad\qquad - J_1(s_1a_1)J_0(s_2a_1).
\end{array}\right\} \quad . \quad (19)$$

As there is to be no current beyond the sheath, $\gamma_3 = 0$, or $H_3 = 0$, at $r = a_3$. This gives

$$B_3 = -A_3 \frac{J_1}{K_1}(s_3 a_3). \quad \ldots \quad (20)$$

This, and the conditions $\gamma_1 = \gamma_2$, and $\rho_3 \Gamma_3 = \rho_2 \Gamma_2$, at $r = a_2$, give us

$$\left. \begin{array}{l} (A_2 J_1 + B_2 K_1)(s_2 a_2) = A_3 \left\{ J_1(s_3 a_2) - \dfrac{J_1}{K_1}(s_3 a_3) K_1(s_3 a_2) \right\}, \\[2mm] (\rho_2 s_2 / \rho_3 s_3)(A_2 J_0 + B_2 K_0)(s_2 a_2) = A_3 \left\{ J_0(s_3 a_2) - \dfrac{J_1}{K_1}(s_3 a_3) K_0(s_3 a_2) \right\} ; \end{array} \right\} \quad (21)$$

whence, eliminating A_3 by division, and putting for A_2 and B_2 their values in terms of A_1 through (19), we obtain the determinantal equation of the p's for a particular value of m^2. It is

$$\frac{\rho_3 s_3}{\rho_2 s_2} \frac{J_0(s_3 a_2) K_1(s_3 a_3) - J_1(s_3 a_3) K_0(s_3 a_2)}{J_1(s_3 a_2) K_1(s_3 a_3) - J_1(s_3 a_3) K_1(s_3 a_2)}$$

$$= \frac{\dfrac{J_1(s_1 a_1) K_0(s_2 a_1) - (\rho_1 s_1 / \rho_2 s_2) J_0(s_1 a_1) K_1(s_2 a_1)}{(\rho_1 s_1 / \rho_2 s_2) J_0(s_1 a_1) J_1(s_2 a_1) - J_1(s_1 a_1) J_0(s_2 a_1)} J_0(s_2 a_2) + K_0(s_2 a_2)}{\ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots J_1(s_2 a_2) + K_1(s_2 a_2)}, (22)$$

where the dots indicate repetition of the fraction immediately over them.

Before proceeding to practical simplifications, we may in outline continue the process of finding the complete solution to correspond to any given initial state. The m's must be found from the terminal conditions. Suppose, for example, that the wire, of length l, forms a closed circuit, and that the sheath and the dielectric are similarly closed on themselves. Then clearly we shall have Fourier periodic series, with

$$m = 0, \quad 2\pi/l, \quad 4\pi/l, \quad 6\pi/l, \quad \&c.$$

If, again, we desire to make the sheath the return to the wire, without external resistance, join them at the end $z = 0$ by a conducting-plate of no resistance, placed perpendicular to the axis ; and do the same at the other end, where $z = l$. This will make

$$\gamma = 0 \text{ at } x = 0 \text{ and at } x = l ;$$

will make the θ's vanish, and

$$m = 0, \quad \pi/l, \quad 2\pi/l, \quad 3\pi/l, \quad \&c.$$

Each of these m's has its infinite series of p's, by the equation (22).

Now, as regards the initial state, the electric field and the magnetic field must be both given. For, although the quantity H, fully expressed, alone settles the complete state of the

system after the first moment, yet at the first moment (when the previously acting impressed forces finally cease) the electric field and the magnetic field are independent. The energy which is dissipated according to Joule's law has two sources, the electric and the magnetic energies. Now we may, by longitudinal impressed force, set up a certain distribution of magnetic energy, but no electric energy. Or, having set up a certain magnetic and a certain electric field by a particular distribution of impressed force, we may alter it in various ways, so as to keep the magnetic field the same whilst we vary the electric field. So both fields require to be known, or equivalent information given.

We may then decompose them into the proper normal systems by means of the universal conjugate property derived from the equation of activity, that of the equality of the mutual electric energy of two complete normal systems to their mutual magnetic energy ('Electrician,' November 27, 1885). Thus, if U_{11} and T_{11} are the doubles of the complete electric and magnetic energies of any normal system, and U_{01} is the mutual electric energy of the initial electric field and the normal electric field in question, and T_{01} is the mutual magnetic energy of the initial magnetic field and the normal magnetic field, we shall have

$$A_1 = \frac{U_{01} - T_{01}}{U_{11} - T_{11}} \quad . \quad . \quad . \quad . \quad (23)$$

as the expression for the value of the coefficient A_1, which settles the actual size of the normal system in question. Equal roots require further investigation. This would complete the theoretical treatment. It is best to use the electric and magnetic forces as initial data in the general case. As regards potentials, we cannot express the electric energy in terms of merely the electric potential and the electrification, but require to use also the vector potential $\mathbf{Z}$ and the magnetic current.

Now there are several important practical simplifications. Suppose, first, that the thickness of the sheath is only a small fraction of its distance from the axis. Then it may be treated as if it were infinitely thin, making the sheath a linear conductor; of course its resistance may remain the same as if of finite thickness. Let a_4 be the very small thickness of the sheath, then the big fraction on the left side of (22) will become

$$\frac{(J_0 + s_3 a_4 J_1) K_1 - (K_0 + s_3 a_4 K_1) J_1}{(J_1 + s_3 a_4 J_2) K_1 - (K_1 + s_3 a_4 K_2) J_1} (s_3 a_3)$$

$$= -\frac{1}{s_3 a_4} \frac{J_1 K_0 - J_0 K_1}{J_2 K_1 - J_1 K_2} (s_3 a_3) = -\frac{1}{s_3 a_4};$$

wherein J_2 and K_2 are derived from J_1 and K_1 as the latter are derived from J_0 and K_0. So the left side of (21) will become

$$-\frac{\rho_3 s_3}{\rho_2 s_2}\frac{1}{s_3 a_4} = -\frac{\rho_3}{\rho_2 a_4 s_2}. \quad . \quad . \quad . \quad (24)$$

The inductivity of the sheath is now of no importance. Being on the outer edge of the magnetic field, the thinness of the sheath makes its contribution to the magnetic energy be diminished indefinitely.

Again, in important practical cases, the resistance of the return is next to nothing in comparison with that of the wire. Then put $\rho_3 = 0$ in (21). This makes the left side vanish, and then we sweep away the denominator on the right side, and get the determinantal or differential equation

$$0 = \frac{J_1(s_1 a_1)K_0(s_2 a_1) - (\rho_1 s_1/\rho_2 s_2)J_0(s_1 a_1)K_1(s_2 a_1)}{(\rho_1 s_1/\rho_2 s_2)J_0(s_1 a_1)J_1(s_2 a_1) - J_1(s_1 a_1)J_0(s_2 a_1)} J_0(s_2 a_2) + K_0(s_2 a_2).\,(25)$$

Although we may have the return of nearly no resistance and yet of low conductivity (as in the case of the Earth), yet it cannot be quite zero without infinite conductivity, which is what is here assumed. The result is that we shut out the return conductor from participation, except superficially, in the phenomena. (24) will result from the condition $\rho_2\Gamma_2 = 0$, or $\Gamma_2 = 0$, at $r = a_2$; that is, no tangential current, or electric force, in the dielectric close to the sheath. If there could be any, it would involve infinite current-density in the sheath. As it is, there is none, and the return-current has become a mere abstraction, to be measured by the tangential magnetic force divided by 4π, and turned round through a right angle on the inner boundary of the sheath. In a similar manner, if we make the wire infinitely conducting (or of infinitely great inductivity either) the wire will be shut out. Then the magnetic and electric fields are confined to the dielectric only, and we shall have purely wave-propagation, unless it be a conductor as well.

Now, with the return of no resistance, let the dielectric be nonconducting and the wire non-dielectric, or $c_1 = 0$, $k_2 = 0$. The most important simplification arises from the smallness of $s_2 a_2$. For we have

$$-s_2^2 = \mu_2 c p^2 + m^2.$$

If the length l of the line is a large multiple of the greatest transverse length a_2 we are concerned with, m^2 is made a small quantity—very small when the line is miles in length, except in case of the insignificant terms involving large multiples of π in $m = n\pi/l$. Again, $(\mu_2 c)^{-\frac{1}{2}}$ is the speed of light

through the dielectric, so that unless p be extravagantly large $\mu_2 c p^2$ is exceedingly small also. Thus, with moderate distance of return-current, $s_2 a_2$ is in general exceedingly small.

Therefore in the expressions (17) take first terms only, making

$$\begin{aligned}
\mathrm{J}_0\,(s_2 r) &= 1, & \mathrm{J}_1(s_2 r) &= \tfrac{1}{2} s_2 r\,; \\
\mathrm{K}_0(s_2 r) &= \log s_2 r\,; & \mathrm{K}_1(s_2 r) &= -\,(s_2 r)^{-1}.
\end{aligned} \right\} \quad . \quad (26)$$

These, used in (25), bring it down to

$$\log \frac{a_2}{a_1} . \mathrm{J}_1(s_1 a_1) = \frac{c p s_1}{4\pi k_1 a_1 s_2^2} \mathrm{J}_0(s_1 a_1)\,; \quad . \quad . \quad . \quad (27)$$

concerning which, so far as numerical accuracy is concerned, the only assumption made is that the return has no resistance.

We have now the following complete normal system:—

$$\left.\begin{aligned}
\mathrm{H}_1 &= \mathrm{A}\mathrm{J}_1(s_1 r) \cos\,(mz+\theta)\epsilon^{pt}, \\
4\pi\gamma_1 &= \mathrm{A}\mathrm{J}_1(s_1 r) m \sin\,(mz+\theta)\epsilon^{pt}, \\
4\pi\Gamma_1 &= \mathrm{A}\mathrm{J}_0(s_1 r) s_1 \cos\,(mz+\theta)\epsilon^{pt}, \\
\mathrm{H}_2 &= \mathrm{B}(s_2^2 r)^{-1} \cos\,(mz+\theta)\epsilon^{pt}, \\
4\pi\gamma_2 &= \mathrm{B}(s_2^2 r)^{-1} m \sin\,(mz+\theta)\epsilon^{pt}, \\
4\pi\Gamma_2 &= \mathrm{B} \log\,(a_2/r) \cos\,(mz+\theta)\epsilon^{pt},
\end{aligned}\right\} \quad . \quad . \quad (28)$$

where $\mathrm{B} = \mathrm{A}(\rho_1 s_1/\rho_2)\mathrm{J}_0(s_1 a_1) \div \log\,(a_2/a_1)$.

The longitudinal current and electric force in the dielectric vary as the logarithm of the ratio a_2/r, vanishing at $r = a_2$. The radial components vary inversely as the distance. Numerically considered, the longitudinal electric force is negligible against the radial, which is important as causing the electrostatic retardation on long lines. But, theoretically, the longitudinal component of the electric force is very important when we look to the physical actions that take place, as it determines the passage of energy from the dielectric, its seat of transmission along the wire, into the conductor, where it is dissipated.

Regarding (28), however, it is to be remarked that, on account of the approximations, the dielectric solutions do not satisfy the fundamental equation (6). Applying it, we get $\Gamma = 0$. But the other fundamental (7) is satisfied. To satisfy (6), take

$$\mathrm{K}_1(s_2 r) = -\,(s_2 r)^{-1} + \tfrac{1}{2} s_2 r\,(\log s_2 r - 1);$$

leading to the determinantal equation

$$\log \frac{a_2}{a_1} . \mathrm{J}_1(s_1 a_1) = \frac{\rho_1 s_1}{\rho_2} \mathrm{J}_0(s_1 a_1) \left\{ \frac{1}{s_2^2 a_1} + \tfrac{1}{2} a_1 \left(\log \frac{a_2}{a_1} + 1 \right) \right\},$$

and requiring us to substitute

$$(s_2^2 r)^{-1} + \tfrac{1}{2} r \log (a_2/r) + \tfrac{1}{2} r$$

for $(s_2^2 r)^{-1}$ in the H_2 and γ_2 formulæ in (28). Then (6) is nearly satisfied, and is quite satisfied if we change the last term in the last expression to $\tfrac{1}{4} r$. But the other fundamental is violated.

Now in (27) take $m=0$, making $-s_2^2 = \mu_2 c p^2$, and bringing (27) down to

$$\tfrac{1}{2} s_1 a_1 J_0(s_1 a_1) = - \frac{L_0}{R_0} p J_1(s_1 a_1); \quad . \quad . \quad . \quad (29)$$

where

$$L_0 = 2\mu_2 \log (a_2/a_1),$$

the coefficient of self-induction of the surface-current, and

$$R_0 = (\pi k_1 a_1^2)^{-1},$$

the resistance of the wire, both per unit length of wire ; so that L_0/R_0 is the time-constant of the linear theory, on the supposition that the resistance of the wire fully operates, although the current is confined to the surface. This case of $m=0$ is appropriate when the line is so short that the electro-static induction is really negligible in its effects on the wire-current. In fact we shall arrive at (29) from purely electro-magnetic considerations, with $c=0$ everywhere. But it is also the proper equation in the $m=0$ case when the electro-static retardation is not negligible. It must be taken into account, for instance, in the subsidence of an initially steady current, independently of the electrostatic charge.

Equation (22) in powers of p, by means of $\tfrac{1}{4} s_1^2 a_1^2 = -\mu_1 p/R_0$ We get

$$1 + \frac{\mu_1 p}{R_0} + \tfrac{1}{4}\left(\frac{\mu_1 p}{R_0}\right)^2 + \ldots = - \frac{L_0 p}{R_0}\left(1 + \frac{\mu_1 p}{2R_0} + \ldots\right). \quad (30)$$

Taking first powers only, we get

$$-p^{-1} = (\mu_1 + L_0)/R_0 ;$$

which is greater than the linear theory time-constant of the wire by the amount $\tfrac{1}{2}\mu_1/R_0$, since $\tfrac{1}{2}\mu_1$ is the coefficient of self-induction per unit length of wire when the return-current is upon its surface.

But taking second powers as well, we get, if $L = \tfrac{1}{2}\mu_1 + L_0$,

$$-p^{-1} = L/R_0 \text{ and } \tfrac{1}{2}\mu_1/R_0,$$

of which the first is exactly the linear-theory value. The real time-constant of the first normal system of current, therefore, exceeds the linear-theory value by an amount which is less

than $\frac{1}{2}\mu_1/R_0$, when the return is so distant, or the retardation $(\mu_1 k_1 a_1^2)$ of the wire is so small that a steady current subsides with very nearly uniform current-density, being very slightly less at the boundary than at the axis. It is not, however, to be inferred that the subsidence of the " current in the wire " is delayed. It is accelerated, at least at first.

(29) may be written

$$(\mu_1/L_0)J_0(s_1 a_1) = \tfrac{1}{2} s_1 a_1 J_1(s_1 a_1), \quad . \quad . \quad . \quad (31)$$

the appropriate form when a full investigation is desired. Draw the curves $y_1 =$ right member, and $y_2 =$ left member, the abscissa being $s_1 a_1$. Their intersections will give the values of $s_1 a_1$ satisfying (31). The first root has been already considered, when μ_1/L_0 is very small. The rest, under the same circumstances, will be nearly those of $J_1(s_1 a_1)=0$. But if the wire is of iron μ_1/L_0 may be very large, and there will be no approach to the linear theory. Many normal systems must be taken into account to get numerical solutions. Similarly if the sheath be close to the wire, whether it be magnetic or not.

Electrostatic charge being ignored, join the wire and sheath to make a closed circuit, in which insert a steady impressed force e at time $t=0$. Let Γ be the current at distance r from the axis at time t. (There is no γ now). The rise of Γ to the final steady value, say Γ_0, is given by

$$\Gamma = \Gamma_0 \left\{ 1 - \Sigma \frac{2}{s_1 a_1} \frac{J_0(s_1 r)\epsilon^{pt}}{J_1(s_1 a_1)(1+s_1^2 q^2)} \right\}, \quad . \quad (32)$$

where $q = L_0 a_1/2\mu_1$. The values of $s_1 a_1$ are to be got by (31).

The total current C, or the current in the wire, in ordinary language, rises thus to its final value C_0:—

$$C = C_0 \left\{ 1 - \Sigma \left(\frac{2}{s_1 a_1} \right)^2 \frac{\epsilon^{pt}}{1+s_1^2 q^2} \right\}. \quad . \quad . \quad (33)$$

The boundary condition of Γ is that, at $r = a_1$,

$$\Gamma + q\frac{d\Gamma}{dr} = 0, \quad \therefore \frac{J_0}{J_1}(s_1 a_1) = s_1 q. \quad . \quad . \quad (34)$$

Considering the first term only in the summation in (33), as may be done except in the first stage of the rise, when the linear theory is nearly followed, put $-p^{-1} = (L+L_1)/R_0$, where L_1 must be very small compared with L ; then

$$C = C_0 \left\{ 1 - \frac{(L+L_1)^2 \epsilon^{pt}}{L^2 + \mu_1 L_1 + \tfrac{1}{4}\mu_1^2} \right\}.$$

When the current is started, by a steady impressed force in

the coil circuit, in a long solenoidal coil of small thickness, containing a solid conducting-core, the magnetic force in the core rises in the same manner as the current in the wire, according to (32); as the boundary condition of the magnetic force is of the same form as (34), q being then a function of the number of windings, &c.

There is also the water-pipe analogy, which is always turning up. This I made use of in the 'Electrician,' July 12,1884. Water in a round pipe is started from rest and set into a state of steady motion by the sudden and continued application of a steady longitudinal dragging or shearing-force applied *to its boundary*, according to the equation (32). This analogue is useful because every one is familiar with the setting of water in motion by friction on its boundary, transmitted inward by viscosity.

Graphically representing (32), abscissæ the time, and ordinates Γ, at the centre, intermediate points, and the boundary, by what we may call the arrival-curves of the current, and comparing them with

$$\Gamma = \Gamma_0(1 - \epsilon^{-R_0 t/L}),$$

the linear-theory arrival-curve at all parts of the wire, we may notice these characteristics. The current rises much more rapidly at the boundary than according to the linear theory, at first, but much more slowly in the later stages. Going inward from the boundary we find that an inflection is produced in the arrival-curve near its commencement; the rapid rise being delayed for an appreciable interval of time. This dead period is very marked of course at the axis of the wire, there being practically no current at all there until a certain time has elapsed. That the central part of the wire is nearly inoperative when rapid reversals are sent is easily understood from this, or perhaps more easily by the use of the water-pipe analogue. Some curves of (32), for two special values of q, I gave in the 'Electrician,' September 6, 1884.

Let there be a simple harmonic impressed force $e \sin nt$ in the circuit of wire and sheath, with no external resistance, making a total circuit-resistance R. (I translate the core solution into the wire solution.) The boundary condition is

$$\frac{e \sin nt}{R \pi a_1^2} = \Gamma + q \frac{d\Gamma}{dr}, \quad \cdots \quad (35)$$

and the solution is

$$\Gamma = \frac{e}{R \pi a_1^2} (P_0^2 + Q_0^2)^{-\frac{1}{2}} \{(P_0 M + Q_0 N) \sin nt + (P_0 N - Q_0 M) \cos nt\} ; \quad (36)$$

where M and N are the following functions,

$$M = \tfrac{1}{2} J_0(xr \sqrt{i}) + \tfrac{1}{2} J_0(xr \sqrt{-i}),$$
$$N = \tfrac{1}{2} i J_0(xr \sqrt{i}) - \tfrac{1}{2} i J_0(xr \sqrt{-i}),$$

$\left. \right\}$, . . . (37)

i standing for $\sqrt{-1}$, and x for $\sqrt{4\pi\mu_1 k_1 n}$. Also

$$P = M + qM', \quad Q = N + qN', \quad \ldots \quad (38)$$

the $'$ denoting differentiation to r. In (36) M and N have the values at distance r, and P_0, Q_0 the values at $r = a_1$, the boundary.

We have

$$P^2 + Q^2 = M^2 + N^2 + 2q(MM' + NN') + q^2(M'^2 + N'^2). \quad (39)$$

If $y = (xr)^4 = (4\pi\mu_1 k_1 r^2 n)^2$, we have the following series:—

$$M^2 + N^2 = 1 + \frac{y}{2 \cdot 4^2}\left(1 + \tfrac{1}{2}\frac{y}{6 \cdot 8^2}\left(1 + \tfrac{1}{3}\frac{y}{10 \cdot 12^2}\left(1 + \tfrac{1}{4}\frac{y}{14 \cdot 16^2}\left(1 + \cdots,\right.\right.\right.\right.$$

$$M'^2 + N'^2 = \frac{y}{4r^2}\left(1 + \frac{3y}{4^2 6^2}\left(1 + \frac{3\tfrac{1}{3}y}{8^2 10^2}\left(1 + \frac{3\tfrac{1}{2}y}{12^2 14^2}\left(1 + \frac{3\tfrac{3}{5}y}{16^2 18^2}\left(1 + \cdots,\right.\right.\right.\right.\right.$$

$$MM' + NN' = \frac{y}{16r}\left(1 + \frac{6y}{6^2 8^2}\left(1 + \frac{5y}{10^2 12^2}\left(1 + \frac{4\tfrac{2}{3}y}{14^2 16^2}\left(1 + \frac{4\tfrac{1}{2}y}{18^2 20^2}\left(1 + \cdots.\right.\right.\right.\right.\right.$$

$\left. \right\}$ (4

These are suitable for calculating the amplitude of Γ or of C when y is not a very large quantity. The wire current C is given by

$$C = -\frac{ea_1}{2l\mu_1 n}\left(\frac{M'^2 + N'^2}{P^2 + Q^2}\right)^{\frac{1}{2}}\sin\left(nt - \tan^{-1}\frac{QN' + PM'}{PN' - QM'}\right), \quad (41)$$

where P, Q, M, N, M', N' have the boundary values. As for M and N themselves, their expansions are

$$M = 1 - \frac{y}{2^2 4^2} + \frac{y^2}{2^2 4^2 6^2 8^2} - \cdots,$$
$$N = \frac{y^{\frac{1}{2}}}{2^2} - \frac{y^{\frac{3}{2}}}{2^2 4^2 6^2} + \frac{y^{\frac{5}{2}}}{2^2 4^2 6^2 8^2 10^2} - \cdots.$$

$\left. \right\}$. (42)

But these series are quite unsuitable when y is very large. Then use the approximate formulæ

$$J_0(sr) = \left(\frac{2}{\pi s r}\right)^{\frac{1}{2}}\cos\left(sr - \frac{\pi}{4}\right),$$
$$J_1(sr) = \left(\frac{2}{\pi s r}\right)^{\frac{1}{2}}\cos\left(sr - \frac{3\pi}{4}\right),$$

$\left. \right\}$ . . (43)

which make, if $f = y^{\frac{1}{2}}$

$$\left.\begin{array}{l}
M^2 + N^2 = J_0(f\sqrt{i})J_0(f\sqrt{-i}) = \epsilon^{f\sqrt{2}}/2\pi f, \\
M'^2 + N'^2 = f\epsilon^{f\sqrt{2}}/2\pi r^2, \\
MM' + NN' = \epsilon^{f\sqrt{2}}/2\pi r\sqrt{2}.
\end{array}\right\} \quad . \quad (44)$$

In the extreme, very high speed, or large retardation, or both combined, making y very great, the amplitude of the wire current C tends to be represented by

$$e/L_0 ln; \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

showing that the current is stronger than according to the linear theory, and far stronger in the case of an iron wire, or very close return.

The amplitude of the current-density at the axis, under the same circumstances,

$$\frac{e}{R\pi a_1^2 L_0} \cdot \frac{2\mu}{a_1} \cdot \left(\frac{2\pi a_1^2}{f\epsilon^{f\sqrt{2}}}\right)^{\frac{1}{2}}, \quad . \quad . \quad . \quad (46)$$

which is of course excessively small. On the other hand, the boundary current-density amplitude is

$$\frac{(2\mu_1/a_1)e}{R\pi a_1^2 L_0(4\pi\mu_1 k_1 n)^{\frac{1}{2}}} = \frac{e}{L_0 l a_1}\left(\frac{\mu_1 k_1}{\pi n}\right)^{\frac{1}{2}}, \quad . \quad . \quad (47)$$

which may be greater than the linear-theory amplitude.

Analogous to this, the amplitude of the current in a coil due to a S.H. impressed force in the coil-circuit is greatly increased by allowing dissipation of energy by conduction in a core placed in the coil, when the corresponding y is great, a large core, high inductivity, &c.; that is, the inertia or retarding-power of the electromagnet is greatly reduced, so far as the coil-current is concerned. This is, in a great measure, done away with by dividing the core to stop the electric currents, when the linear theory is approximated to.

If $y = 1600$, the axial is about one fourteenth of the boundary-current amplitude. To get this in a thick copper wire of 1 centim. radius, a speed of about 850 waves per second would be required. But in an iron rod of the same size, if we take $\mu_1 = 500$, only about $8\frac{1}{2}$ waves per second would suffice.

Returning to the former expressions, if we go only as far as n^6, the amplitude C_0 of the wire current is given by $C_0 = e/R''l$; where the square of R'', which is the apparent resistance, or the impedance, per unit length of wire, is given by

$$R''^2 = R_0^2 + L^2 n^2 + \frac{R_0^2 g}{6} - \frac{R_0^2 g^2}{24}\left(\frac{L}{\mu_1} + \frac{1}{10}\right) + \frac{R_0^2\, g^3}{48 \cdot 90}\left(13\frac{L}{\mu_1} + \frac{79}{56}\right) - \ldots, \quad (48)$$

where $g = (\mu_1 n/R_0)^2$, and R_0 and L have the former meanings.

When only the total current is under investigation, the method followed by Lord Rayleigh (Phil. Mag. May 1886) possesses advantages. I find it difficult, however, to understand how the increased resistance can become of serious moment. For, above a certain speed, the current amplitude is increased ; whilst, below that speed, its reduction, from that given by the linear theory, appears to be, in copper wires, quite insignificant in general.

The remainder of this paper I must postpone, it being already of a reasonable length.

XV. *On a new Hyperbolagraph.* By H. Cunynghame[*].

IT is a not unfrequent want to be able to find the rectangle of greatest or least area contained between a curve and any given rectangular coordinate axes. In several problems connected with motion and pressure in steam-engines this is very useful ; and even in political economy the graphic representations of monopoly-curves depend on the maxima and minima of this nature..

For the solutions of such problems it is often very useful to be able to describe any rectangular hyperbola, the axes being given. To effect this, I have constructed a machine which is capable of drawing these curves with considerable accuracy. It depends on a mathematical property of the rectangular hyperbola, which, so far as I am aware, is new. From a fixed point O let a line O P be drawn to meet a fixed line A B in P. Take P Q perpendicular to A B, and make $OP + PQ = a$ constant length. Then Q is the locus of a rectangular hyperbola whose asymptotes are equally inclined to A B, and such that if the said asymptotes be taken as axes, $xy = 2(OM)^2$. This is easy to prove ; for if $PM = x$, $PQ = y$, and $OQ = b$ and $OM = A$; then

$$y = a - OP,$$
$$= a - \sqrt{a^2 + x^2},$$

or

$$a^2 - 2ay + y^2 = a^2 + x^2,$$
$$y^2 - 2ay \quad = x^2,$$

which is the equation to a rectangular hyperbola.

The line O P Q, instead of being made of thread, is made of a
fine wire of steel wound round a roller at O, and given one
complete turn round the roller at P, fixed to the movable
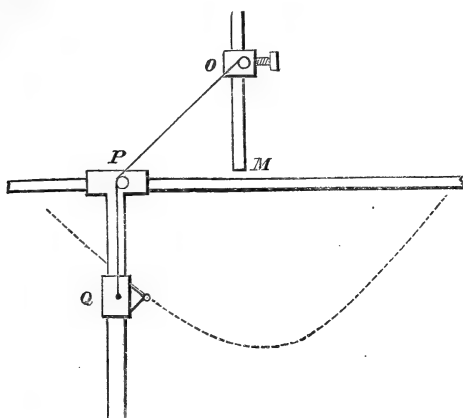
T-square P Q. It is kept stretched by means of a string
and weight, or a spring pulling it away (not shown in the
figure).

This use of rollers and a fine steel wire, or a band, is very
useful in all instruments depending on the use of strings.   As
a substitute for it, a flat band of steel or copper wire may

sometimes also be employed.   It
will of course be noticed that
the rollers form a compensation
arrangement, for as much as is
rolled on to one roller is rolled
off the other.   It will also not
fail to be noticed, that an ellipto-
graph could be constructed in

this manner by the use of a steel wire and rollers ; the rollers
being arranged so as to compensate one another, so that when
much wire was rolled up on one, less would be rolled up on
the others. This principle has hardly been sufficiently used
up to now, and is worthy of attention.   In many other instru-
ments depending on the length of strings, the direction of the
forces altering, there is unequal strain ; but in this instrument
(as was pointed out by Mr. Boys) there will be no such
tendency, as the string is always kept stretched by a constant
weight.
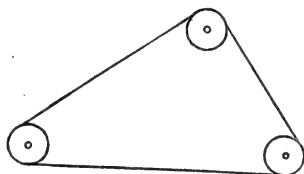
XVI. *Note on the Induction of Electric Currents, in an Infinite Plane Current Sheet, which is rotating in a Field of Magnetic Force. By* A. B. Basset, *M.A.*[*]

1. THE determination of the currents induced in a thin plane conducting sheet of infinite extent, which is rotating about an axis perpendicular to itself in a field of magnetic force, may be effected either by employing the equations of electromotive force referred to moving axes, or by dealing with the problem directly by means of the method of images. The former method has been employed by Maxwell[†] and Prof. C. Niven[‡], and is probably the preferable one when the currents cannot be conveniently represented by a system of images ; but when the magnetic field is produced by a system of magnets or currents, the problem can be easily solved by finding an analytical expression for the magnetic potential of the moving trail of images, to which the currents are equivalent.

In the present note I have employed the latter method of dealing with the problem, and have thence deduced Niven's results ; and I have also worked out the solution in the case of a current sheet rotating about an axis, in the presence of a short magnet which is fixed at right angles to the axis of revolution.

2. To solve the problem by means of images, let us choose the instant at which we desire to observe the currents as the origin of the time, and reckon the time backwards from this epoch.

Adopting Maxwell's notation, let $F (z, \tau)$ be the value of P' at time τ ago (where $-dP'/\delta z$ is the magnetic potential of the inducing system), and let the system P' be suddenly introduced and kept at rest ; a system of currents will be instantaneously generated for which the value of P (where $-dP/dz$ is the magnetic potential of currents) is $f(z, \tau)$, the form of the function f being determined from the fact that it is the image of F with respect to the sheet.

This system of currents, as soon as it has been generated, will begin to decay, the law of decay being such that, at the end of an interval T,

$$P = f(z + RT, \tau),$$

where $2\pi R$ is the specific resistance of the sheet.

If, therefore, at the end of an interval $d\tau$ the system P' be

removed to the position which it occupied at time $\tau - d\tau$ ago, the value of P corresponding to the sudden introduction and removal of the system P' will be

$$f(z + \mathrm{R}d\tau, \tau) - f(z, \tau) = \mathrm{R}\frac{d}{dz}f(z, \tau)dt.$$

At the instant under consideration (that is, after an interval $\tau - d\tau$), the value of this is

$$\mathrm{R}\frac{d}{dz}f\{z + \mathrm{R}(\tau - d\tau),\ \tau\}\,d\tau$$

$$= \mathrm{R}\frac{d}{dz}f(z + \mathrm{R}\tau, \tau)d\tau.$$

ultimately. Summing up all similar terms between the limits t and 0 of τ, we have finally

$$\mathrm{P} = \mathrm{R}\frac{d}{dz}\int_0^t f(z + \mathrm{R}\tau, \tau)d\tau + f(z, 0),\quad . \quad . \quad (1)$$

the last term being the potential of the currents generated at the instant at which we are observing them.

If the electromagnetic system is rotating about the axis of z in the same direction as that in which ϕ is measured, and with uniform angular velocity ω, the value of $f(z + \mathrm{R}\tau, \tau)$, expressed in cylindrical coordinates, will be

$$f(z + \mathrm{R}\tau, \rho, \phi + \omega\tau)\ ;$$

and the value of P, after the currents have become steady, will be obtained by integrating between the limits ∞ and 0 ; whence

$$\mathrm{P} = \mathrm{R}\frac{d}{dz}\int_0^\infty f(z + \mathrm{R}\tau, \rho, \phi + \omega\tau)d\tau + f(z, \rho, \phi),\quad . \quad . \quad (2)$$

which represents a spiral trail of images.

3. From the foregoing expression for P we can at once obtain Niven's results. For

$$\frac{df}{d\tau} = \mathrm{R}\frac{df}{dz} + \omega\frac{df}{d\phi};\quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\therefore\ \mathrm{R}\frac{d\mathrm{P}}{dz} + \omega\frac{d\mathrm{P}}{d\phi} = \mathrm{R}\frac{d}{dz}\int_0^\infty \frac{df}{d\tau}\,d\tau + \left(\mathrm{R}\frac{df}{dz} + \omega\frac{df}{d\phi}\right)_{\tau=0} = \omega\left(\frac{df}{d\phi}\right)_{\tau=0}.\ (4)$$

since $f = 0$ when $\tau = \infty$.

The last result is perfectly general ; but at the surface,

$$-\frac{d\mathrm{P}}{dz} = 2\pi\Phi,\quad f = -\mathrm{P}',$$

whence equation (4) becomes

$$\omega \frac{d}{d\phi}(P+P')=\sigma\Phi. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The value of the electric potential ψ may be obtained as follows. From Maxwell, art 600,

$$\psi=\psi'-F'\dot{x}-G'\dot{y}-H'\dot{z}\ ; \quad . \quad . \quad . \quad (6)$$

where ψ' is the quantity which appears in art. 658, and which is therefore zero; ψ is the quantity which appears in art. 668[*]; and F', G', H' are the components parallel to moving axes x,y,z of the total vector potential due to the influencing system and the currents. In the present case,

$$\dot{x}=-\omega y, \quad \dot{y}=\omega x, \quad \dot{z}=0\ ;$$

$$\therefore\ \psi=\omega(F'y-G'x)$$

$$=\omega\left(y\frac{d}{dy}+x\frac{d}{dx}\right)(P+P')$$

$$=\omega\rho\frac{d}{d\rho}(P+P') \quad . \quad . \quad . \quad . \quad . \quad (7)$$

at the surface.

If we change the signs of P and P', it will be seen that (5) and (7) agree with Prof. C. Niven's results[†].

4. Another result, which is more convenient for practical purposes, may be obtained as follows.

Taking account of (3), equation (2) may be written

$$P=\int_0^\infty\left(\frac{df}{d\tau}-\omega\frac{df}{d\phi}\right)d\tau+f(z,\rho,\phi)\ ;$$

and since $f(z+R\tau,\rho,\phi+\omega\tau)$ vanishes when $\tau=\infty$,

$$P=-\omega\int_0^\infty\frac{df}{d\phi}\,d\tau,$$

Differentiating with respect to z, we obtain

$$\Omega=-\omega\int_0^\infty\frac{d\Omega'}{d\phi}\,d\tau\ ; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where Ω' is the magnetic potential at the instant under consideration of the currents which were instantaneously generated at time τ ago.

5. Let us now suppose that we have a long thin bar-magnet

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[*] It should be noticed that the two ψ's in Maxwell's investigation do not represent the same quantity.
[†] Phil. Trans. 1881, p. 342.

whose axis is parallel to the axis of revolution, and whose positive pole is situated at a point whose coordinates are $z=h$, $\rho=c$, $\phi=0$: and let us find the magnetic potential of the currents, when the magnet is rotating about the axis of z, in the *same* direction as that in which ϕ is measured.

In this case,

$$\Omega' = \frac{m}{\{(z+\mathrm{R}\tau+h)^2+\rho^2+c^2-2\rho c \cos(\phi+\omega\tau)\}^{\frac{1}{2}}},$$

where m is the strength of the positive pole ; whence by (8),

$$\Omega = -m\omega \frac{d}{d\phi}\int_0^\infty \frac{d\tau}{\{(z+\mathrm{R}\tau+h)^2+\rho^2+c^2-2\rho c \cos(\phi+\omega\tau)\}^{\frac{1}{2}}}.$$

Let

$$Q = \{\rho^2+c^2-2\rho c \cos(\phi+\omega\tau)\}^{\frac{1}{2}} ;$$

Then, since

$$\int_0^\infty \epsilon^{-\lambda v} \mathrm{J}_0(\lambda Q) d\lambda = \frac{1}{(v^2+Q^2)^{\frac{1}{2}}},$$

and

$$\mathrm{J}_0(\lambda Q) = \mathrm{J}_0(\lambda\rho)\mathrm{J}_0(\lambda c) + 2\Sigma_1^\infty \mathrm{J}_n(\lambda\rho)\mathrm{J}_n(\lambda c) \cos n(\phi+\omega\tau),$$

the value of Ω becomes

$$\Omega = -2m\omega \frac{d}{d\phi}\int_0^\infty d\lambda \int_0^\infty \epsilon^{-\lambda(z+\mathrm{R}\tau+h)} \Sigma_1^\infty \mathrm{J}_n(\lambda\rho)\mathrm{J}_n(\lambda c) \cos n(\phi+\omega\tau) d\tau$$

$$= 2m\omega \Sigma_1^\infty n \int_0^\infty \frac{\epsilon^{-\lambda(z+h)}(\mathrm{R}\lambda \sin n\phi + n\omega \cos n\phi)\mathrm{J}_n(\lambda\rho)\mathrm{J}_n(\lambda c)}{\mathrm{R}^2\lambda^2+n^2\omega^2} d\lambda. \quad (9)$$

The forces which act on the pole are given by the following equations :—

$$\left.\begin{aligned}
-\frac{1}{c}\frac{d\Omega}{d\phi} &= -\frac{2m\mathrm{R}}{c}\Sigma n^2 \int_0^\infty \frac{\lambda\epsilon^{-2\lambda h}}{\mathrm{R}^2\lambda^2+n^2\omega^2} \mathrm{J}_n^2(\lambda c) d\lambda, \\
-\frac{d\Omega}{dz} &= 2m\omega^2\Sigma n^2 \int_0^\infty \frac{\lambda\epsilon^{-2\lambda h}}{\mathrm{R}^2\lambda^2+n^2\omega^2} \mathrm{J}_n^2(\lambda c) d\lambda, \\
-\frac{d\Omega}{d\rho} &= -2m\omega^2\Sigma n^2 \int_0^\infty \frac{\lambda\epsilon^{-2\lambda h}}{\mathrm{R}^2\lambda^2+n^2\omega^2} \mathrm{J'}_n(\lambda c)\mathrm{J}_n(\lambda c) d\lambda.
\end{aligned}\right\} \quad (10)$$

Now we have supposed the magnetic pole to be rotating in the same direction as that in which ϕ is measured, therefore the first equation indicates a force tending to oppose the motion of the magnet ; hence the magnet exerts a force on the sheet tending to pull it round in the direction of rotation. If therefore the magnet is fixed and the sheet be made to rotate, the magnet will experience a dragging force parallel to the direction of motion of the sheet.

The second equation represents a repulsive force acting from the disk ; and the third equation represents a force acting towards the axis of the disk.

These results are equivalent to those obtained by Maxwell and observed by Arago. (see art. 669).

If the magnet were not sufficiently long to enable us to leave the action of the negative pole out of account, the magnetic potential of the currents excited by the latter will be found by changing m into $-m$, h into $h+a$, where a is the length of the magnet ; and the combined effect of the two poles will be obtained by adding the results.

It is, however, simpler to place a *short* magnet, of length $2c$, at right angles to the axis of revolution, and consider the combined effect due to both poles. The magnetic potential of the currents due to the action of the negative pole will be obtained in this case by changing m into $-m$, and ϕ into $\pi + \phi$; whence the expressions for the magnetic forces acting on the positive pole will be found from equations (10) by changing m into $2m$, and n into $2n+1$, and effecting the summation with respect to n from ∞ to 0.

The calculation can then be effected as follows :—Expand the Bessel's functions in powers of λc ; the expressions for the forces will then assume the form of series every term of which is of the form

$$\int_0^\infty \frac{\lambda^p \epsilon^{-2\lambda h} d\lambda}{R^2\lambda^2 + (2n+1)^2\omega^2},$$

where p and n are positive integers. Now

$$\int_0^\infty \frac{\epsilon^{-2\lambda h} d\lambda}{R^2\lambda^2 + (2n+1)^2\omega^2} = \frac{1}{(2n+1)R\omega} \left\{ \cos q\left(\frac{\pi}{2} - \text{Si } q\right) + \sin q\, \text{Ci} q \right\}.$$

where $q = 2(2n+1)h\omega/R$. Hence, by differentiating with respect to h, the first-mentioned integral can be expressed in terms of the sine and cosine integrals, the values of which have been calculated by Mr. Glaisher, whence numerical values of the magnetic forces might be found.

If we neglect powers of c higher than the fifth, the magnetic forces in the ϕ, z, and ρ directions will be found to be respectively equal to

$$m R \int_0^\infty \frac{\lambda^3 \epsilon^{-2\lambda h}}{R^2\lambda^2 + \omega^2}\left(c - \frac{\lambda^2 c^3}{4} + \frac{5\lambda^4 c^5}{192}\right)d\lambda + \frac{m R c^5}{64}\int_0^\infty \frac{\lambda^7 \epsilon^{-2\lambda h} d\lambda}{R^2\lambda^2 + 9\omega^2};$$

$$m\omega^2 \int_0^\infty \frac{\lambda^3 \epsilon^{-2\lambda h}}{R^2\lambda^2 + \omega^2}\left(c^2 - \frac{\lambda^2 c^4}{4}\right)d\lambda;$$

$$-m\omega^2 \int_0^\infty \frac{\lambda^2 \epsilon^{-2\lambda h}}{R^2\lambda^2 + \omega^2}\left(c - \frac{\lambda^2 c^3}{2} + \frac{5\lambda^4 c^5}{64}\right)d\lambda - \frac{3m\omega^2 c^5}{64}\int_0^\infty \frac{\lambda^6 \epsilon^{-2\lambda h} d\lambda}{R^2\lambda^2 + 9\omega^2}.$$

XVII. *On an Electric-light Fire-damp Indicator.*
By Walter Emmott *and* William Ackroyd. [*]

THE Royal Commission on Accidents in Mines point out, in their recently issued Report, a serious objection to the use of the electric light in mines, notwithstanding its many other great advantages, in that the light of an incandescent lamp being produced within a vacuum cannot admit of any device for the indication of fire-damp such as is employed in the Davy for example. This difficulty was

Fig. 1.
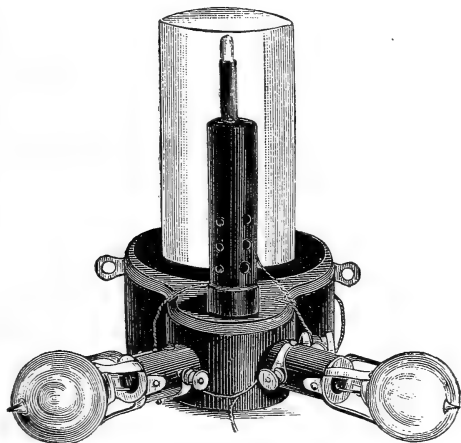
experienced by one of us in the course of an installation of the electric light in the Lofthouse pit, Wyke, Yorks, in the summer of 1885 ; and we have since made a series of experiments with the object of devising a method of making the electric light an indicator of fire-damp. The apparatus placed before the Physical Society is the outcome of our work. It consists of two incandescent lamps, one with white glass and the other with red, and other necessary adjuncts, such that in an ordinary atmosphere the white incandescent lamp alone shines, but in fire-damp the white lamp goes out and the red one begins to emit its light. This is effected as follows :—A porous pot of unglazed hard-baked porcelain is joined by air-tight connections to a tube a portion of which is represented by TT^1, fig. 2. This tube is of such an internal diameter that it will readily admit of being sealed with a small quantity of mercury, Hg. A platinum wire runs the whole length of the tube and is

* Communicated by the Physical Society: read June 12, 1886.

connected with one of the poles of the battery B or other source of electricity. Two other platinum wires in the tube run parallel with this for part of the way, as in fig. 2, and each is connected with a lamp. The lamps W and R are joined, and a branch wire connects them to the other pole of the battery. In fig. 2 the current is represented as flowing through W; when from diffusion in an atmosphere of fire-damp, the conducting plug Hg is driven up to T^1, the current will flow through R, and the red light may then be taken to indicate the presence of fire-damp.

Fig. 2.
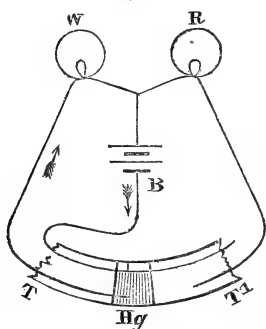
Fig. 3.
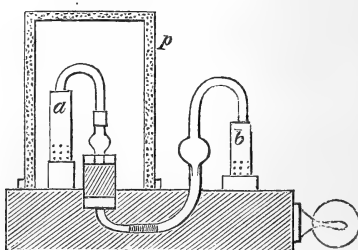
p. Porous pot.
a and *b*, desiccating tubes.

The wires being within the tube, one or other of the lamps must always be shining so long as there is a current, whether the apparatus be in an atmosphere of fire-damp, choke-damp, or air; and to prevent the mercury being driven out of the tube by too much pressure, bulbs are arranged on either side as in fig. 3, which presents a diagrammatic view of the apparatus. We have found an internal diameter of tubing of about 3 millim. best adapted for ensuring easy mobility of the mercury. The presence of the wires within the tube has interfered with the perfection of the seal; this we have overcome by the introduction of a little concentrated sulphuric acid, which also serves the purpose of preventing sparking and of lubricating the interior. The use of sulphuric acid necessitates the addition of desiccators *a* and *b*, fig. 3, to each end of the tube; but in cases where it has been found advisable not to use sulphuric acid, both the acid and the desiccators have been dispensed with by slightly modifying the arrangement of the wires at the lower part of the tube. With this form of apparatus we are readily able to detect the presence of 5 per cent. of coal-gas in a mixture of this gas with air; and with a mercury seal of less weight and closer proximity of the wires at T and T^1 (fig. 2), it appears possible to get any required degree of sensitiveness. It is proposed to have the apparatus fixed in the main roads and hauling roads in pit installations.

XVIII. *On certain Modifications of a Form of Spherical Integrator.* By FREDERICK JOHN SMITH, *B.A. Oxon.**

WHILE working at the subject of Dynamometric Measurements, a great number of different forms of mechanical integrators were attached to a transmission-dynamometer (Phil. Mag. vol. xv. p. 87). In one of these the small disk of a Morin integrator (Phil. Mag. vol. xvii. p. 59) was replaced by a sphere carried between four little cylinders; the sphere was made of phosphorbronze (Ashmol. Soc. 1884).

As long as the dynamometer was driven with but little variation, the results were satisfactory; but as soon as one had to deal with quick variation, either of tension of belt or of velocity, it was found that the moment of inertia of the sphere was a serious obstacle to accurate results. It appeared probable that no instrument dependent upon the action of gravity could be relied upon. This being the case, it was evident that if a sphere was to be used it ought, in the first place, to be as light

Fig. 1.
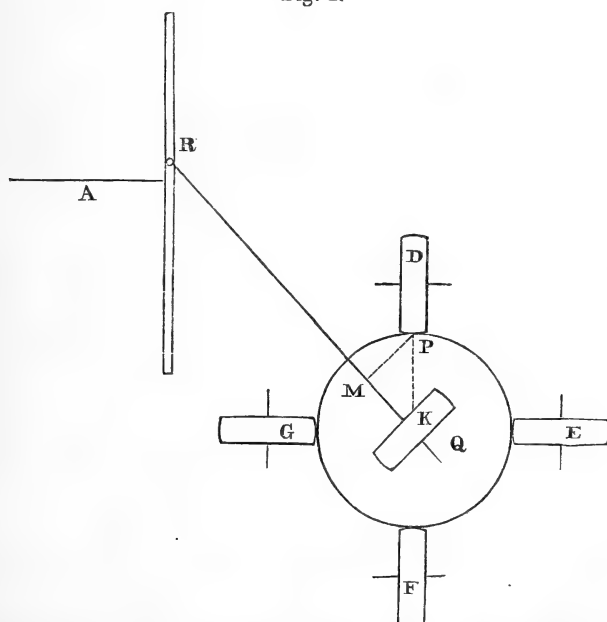
as practicable and, secondly, as firmly held as possible from any slip. These two ends have been arrived at as follows :—The sphere Q is made hollow in order to be light, and it moves between six wheels; of these, four wheels, D E F G (fig. 1), are

* Communicated by the Author.

fixed upon a frame, while two others K L, held in a frame
S M N V (fig. 2) rocking about points at S V, keep the sphere

Fig. 2.
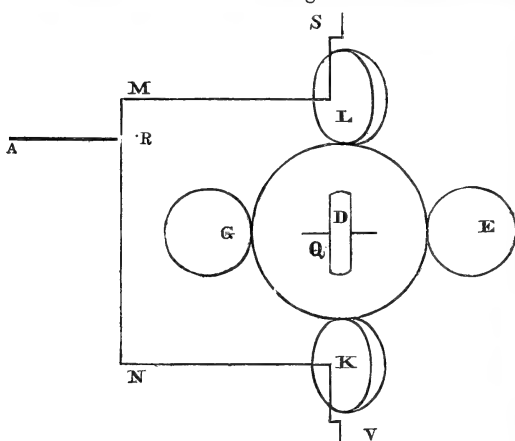
in compression ; the frame is so moved, by means of a slot R
attached to the spring rod of the dynamometer, that the line
P M, and therefore the circle on the sphere rolling upon the
cylinder D, is proportional to the tension of the driving-belt ;
the cylinder K is driven at a rate proportional to the velocity
of the belt-speed, and the cylinder D is attached to a revo-
lution-counter.   When the tension of the belt is constant, then
the revolution of D is proportional to the velocity.   When
the velocity is constant, the revolution is proportional to the
tension of the belt.   When both velocity and tension vary
together, then the revolution of D is proportional to the pro-
duct of velocity and tension which is the work at any instant,
and the number of revolutions of D during any time is pro-
portional to the work done during that time, and shows,
therefore, how much energy has been transmitted by the
dynamometer.

In the figures, for the sake of clearness, the frame which
carries the cylinders is omitted.   The instrument which the
author of this communication has arrived at, is quite unaffected
by any rapid change either of velocity or tension of belt that
may take place while it is running.   The beautiful three-wheel
spherical integrator of Prof. Hele Shaw, which was applied
to the dynamometer, as long as it had to deal with slowly
varying velocity, acted very well indeed ; the idea of changing
the direction of the angle at which the poles of the sphere lie
in is taken from it.

Algiers, March 1886.

XIX. *On hitherto unrecognized Wave-lengths.*
By Prof. S. P. LANGLEY, *Allegheny, Pa.**

[Plates IV.–VI.]

WE have already† presented a description of the method
by which we are able to fix the wave-lengths in the solar
spectrum by direct measurement as far as about 23,000 of
Ångström's scale. At this point the heat in the solar normal
spectrum had become so feeble that it taxed the utmost limits
of our capacity in 1881 to measure it ; for it will be remem-
bered that we are able to study the prismatic spectrum of the
infra-red with comparative ease, because the prism condenses
the heat ; but the grating greatly diffuses and weakens it, so
that, were it due to this cause alone, we should find measure-
ments in this part of the grating spectrum enormously more
difficult than those in the prismatic. But, independently of
this, of the heat which belongs to any ray, our grating in
general employs not over the tenth part. These causes com-
bine to make the heat in certain portions, where we have been
compelled to measure, almost infinitesimal.

We are led to take this labour ; not primarily to settle the
theoretical questions involved in determining the relations
between dispersion and wave-length (though these are most
interesting), but with the object of providing a way which
will hereafter enable any observer to determine the visible or
invisible wave-lengths of any heat, whether from a celestial
or terrestrial source, observed in any prism ; and thus to gain
that knowledge of the intimate constitution of radiant bodies
which an acquaintance with the vibratory period of their
molecules can usually alone afford us. It is this considerable
end—the attempt to open up more fruitful means of research
in the whole unexplored region of infra-red energy, not only
from celestial but from terrestrial sources—which will, we
hope, justify the labour devoted to the following determina-
tions. It may be hoped that wider interest will attach to our
task of demonstrating the character of a certain curve ; when
it is seen that a knowledge of its true form has ceased to be a
matter of abstract speculation only, but will, in connection
with what has already appeared, now introduce to us such
large regions of research as we have just indicated. Over and
above this, however, we shall find our results also affecting
opinion on the theoretical considerations regarding the relation
of wave-length and dispersion just alluded to.

* Communicated by the Author.
† This Journal, March 1884, p. 194.

In previous communications I have given a representation of the solar-heat spectrum terminating near $2^{\mu} \cdot 7$ or $2^{\mu} \cdot 8$, and I have stated that while there were feeble indications of solar heat below this point, yet that the solar radiation beyond seemed sensibly cut off, as though below this were a nearly unlimited cold band. I do not mean then, in saying that solar heat sensibly ceases below this point, to say that absolutely none can exist, but that none, at any rate, does exist sensible to the delicate apparatus with which these first determinations were made, and that none in any case exists of an order in the least comparable with the smaller portions of that already described.

The reader will gather a more clear conception of the difficulty of decision and of the almost infinitesimal amount of this solar heat, if it exist, by looking at Plate IV. fig. 1, in connection with the statement that if there be any solar heat at 4^{μ} the highest ordinate representing it, on the same scale as that shown on the left of the Plate, would at any rate not occupy the thickness of the horizontal line which represents the axis of abscissæ. However, since we are rather inclined to admit, from our final experiments with our latest and most sensitive apparatus, that heat of some kind reappears here (near 4^{μ}), whether from the atmosphere of the sun, or elsewhere, insensible to the most delicate thermopile, and in any case, if it be real, almost infinitesimal in degree, or of the same order of intensity with that in the lunar spectrum, our statement that no sensible solar heat exists in this part of the spectrum must be taken under this qualification.

New Apparatus.

The apparatus for the determination of wave-lengths in connection with the flint-glass prism has been already described[*]. The following is the same in principle, with certain changes to adapt it either to the solar or electric heat. Let S_1 be the first slit (see Plate V). For solar heat it has doubly-moving jaws, controlled by a micrometer-screw, while in the case of the electric arc we use a special form of slit surrounded by water, to be directly described. G is the large concave grating. The massive beam A carries the large rock-salt train S_2, L_1, P, L_2, B. G is fixed at the extremity of the beam, so that its collimating axis coincides with that of L_1; and by means of an automatic apparatus, not shown here, the slits S_1 S_2 are caused to lie always in the same straight line at right angles to G S_1. Under

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[*] This Journal, March 1884, p. 194.

these circumstances, it has been demonstrated by Prof. Rowland that the wave-length of light, passing through the slit S_1 to fall upon the grating and there be diffracted to S_2, is directly proportional to the distance $S_1 \ S_2$. Accordingly, owing to this extremely simple relation, we are able to state at once what invisible ray or rays are at any moment passing through the slit into our rock-salt train. Our engraving represents the arrangement as fitted up for the heat of the electric arc, which is placed immediately in front of the special nozzle *n* carrying the slit S_1. We wish to employ the arc chiefly in the extreme infra-red beyond the solar heat, and where any heat is exclusively minute. The hottest part of the electric arc is found in the pit or crater of the positive carbon, concealed from direct vision, and occupying a space of only 3 or 4 millim. square, even in large arcs. The carbons, then, must be inclined in order that a horizontal beam may escape from this almost hidden crater, which, owing to its small size, should be brought nearly in contact with the slit, in order to utilize the whole of its very minute area, while in this case the inclination of the carbons will prevent such approach. Experiments with various forms of incandescent strips and carbons, directed by clockwork in the ordinary position, have proved the necessity of adopting the special device by which we have finally overcome these difficulties. Figure 3, Plate V., shows in section and in front view a special slit, conical in form, around which a current of water is forced to circulate. Figure 2 shows the carbons on a smaller scale and the apparatus which permits them to be set at any height, inclined at any angle to the vertical, drawn back or approached to any distance. They are usually placed almost in contact with the special slit S_1; and the need of the water circulation is obvious, were it only to prevent the sides of the jaws of the slit from melting, as they would otherwise soon do. There is, however, another necessity for this water-circulation. The need of a slit which may be artificially cooled for measurements in the extreme infra-red of the spectrum from the electric arc, is rendered evident when we state, that for these extreme wave-lengths the arc radiation is comparatively so small, that the heat from the hottest part of the dazzling bright carbon does not very greatly exceed that from a piece of melting ice. If, then, we are to distinguish here between the radiation which passes through the open slit from the incandescent carbon, and that which comes from the adjacent edges of the slit, which inevitably mingles more or less with the former, the difference between the two temperatures must be made as great as possible.

Following now on Plate V. the course of the ray from this electric arc, we observe that it falls on the grating G (to be presently described), which spreads it out into not one but many superposed spectra, distributed along the circumference of a circle whose centre is at O equidistant from G, S_1, and S_2. For clearer illustration, let us suppose ourselves about to measure the heat in some ray of the visible spectrum such as that near D_2 (whose wave-length is nearly $0^{\mu}\cdot6$), and that the line $S_1 S_2$ is a scale of equal parts. In this case, the beam A will be moved so that, while the grating remains at the intersection of N S and G A and normal to the latter, the slit S_2 will be brought close to S_1 in the position $0^{\mu}\cdot6$ on our scale of equal parts, whose zero is at S_1. Here (if we suppose sunlight to be employed) we shall see a brilliant spectrum filled with Fraunhofer lines crossing the front of the plate of the slit S_2. Beyond this, the second, third, and other higher orders of spectra are distributed on the circumference of the circle in which S_2 always lies. Were it our only object to discriminate the heat in this particular visible ray, we should not in this case need the slit S_2 or the prismatic train, but could place the bolometer directly at S_2. Since we make the ordinary use of the slit S_2 however, we suppose ourselves to be determining the prismatic dispersion for a given wavelength; that is, it forms with the prismatic train an approximately homogeneous spectral image at B, which can be viewed or measured with the bolometer, giving the value of n for a known value of λ. For the mere purpose of measuring the heat in the ray, or determining its wave-length here in the visible part, where there is but a single sensible heat-spectrum, we do not need slit S_2 at all; while the refractive index for a glass prism could be as easily determined as that for rock-salt. Besides this, we should find here a relatively abundant heat, and could use so narrow a bolometer as to fix the position by the heat alone quite accurately. Very different, however, are all the conditions if we wish to measure, for example, a wavelength corresponding to 3^{μ} or 4^{μ} in the *invisible* spectrum and in the new region which we are for the first time exploring. Glass is impermeable to this kind of heat, but with our rock-salt train and with the delicate apparatus previously described, there is little difficulty in discriminating it by the bolometer, where the prism alone is employed, and in mapping the deviation of each spectral ray, as shown in plate vi. (Phil. Mag. May 1886). But now that we wish to determine wave-lengths, the conditions are altogether different; for now not only does the grating enormously expand this part of the spectrum and diminish the heat correspondingly, just where that heat is itself

feeblest (so that the heat in parts of this region is something like $\frac{1}{1000}$ part of that in the corresponding prismatic spectrum), but instead of one visible, we have now to deal with numerous invisible spectra, overlapping each other.

Here, then, the slit S_2 has an additional function to fulfil, namely, with the aid of the prism, to discriminate these invisible spectra from each other. Thus in the position actually shown in the drawing, which corresponds to a wave-length of $3^{\mu}\cdot5341$, *i. e.* of $35,341$ of Ångström, we should see the slit covered by a bright spectrum due to several of the higher orders, while we know that energy of the wave-length we are seeking is wholly invisible. If we place a pellet of sodium in our electric arc, we shall see the two sodium-lines on the slit-plate, of which D_2 will fall exactly on the slit, if it be in adjustment, but this sodium-line evidently does not belong to the wave-length we are seeking.

There are, in fact, passing through the same slit and lying superposed on one another by an unavoidable property of the grating, an infinite number of spectra in theory, of which in this case nearly 20 are actually recognizable by photography, by the eye, or by the bolometer; and of which, to consider only those where the wave-length is equal to or greater than that of the sodium-line D_2*, we have six spectra as follows :—

a (visible)	6th spectrum	.	.	D_2	$\lambda = \overset{\circ}{0}\cdot5890$
b ,,	5th ,,	.	.	$\frac{6}{5}D_2$	$0\cdot7068$
c (invisible)	4th ,,	.	.	$\frac{6}{4}D_2$	$0\cdot8835$
d ,,	3rd ,,	.	.	$\frac{6}{3}D_2$	$1\cdot1780$
e ,,	2nd ,,	.	.	$\frac{6}{2}D_2$	$1\cdot7670$
f ,,	1st ,,	.	.	$6D_2$	$3\cdot5341$

It is in this invisible underlying 1st spectrum, buried, so to speak, beneath five others, of which three are themselves invisible also, that lies the wave-length we are seeking ; consequently there are (to consider no others) at least six qualities of heat, of six distinct refrangibilities, whose wave-lengths are equal to or greater than that of D_2, which pass simultaneously through the slit S_2. They pass through the prism, and on looking through a telescope occupying the position of the bolometer-tube, we shall, by suitably directing the arm of the spectroscope, see the light from the sixth one at a. Its wave-length will be $0^{\mu}\cdot5890$, corresponding to a measured deviation (in the case of the rock-salt prism of an angle of $60°\ 00'\ 00''$ and a tem-

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* We have heretofore adopted Ångström's notation in calling the more-refrangible sodium-line " D_1,". We shall hereafter, however, in conformity with the now more general usage, call this line, whose wave-length in Ångström is 5889, " D_2,". The corrections to Ångström are due to the researches of Messrs. Peirce and Rowland.

perature of 20° C.) of 41° 05′ 40″. Now, on replacing the telescope by the bolometer, the bolometer-wire will feel this same ray which the eye has just recognized by its light; and, if the galvanometer be in a sensitive condition, the image will be thrown by the heat off the scale, while a little on either side of this position no indication will be given. The beam and the slit S₂ remaining in the same position, let us next suppose that the bolometer-arm is carried towards *b*, in the direction of B. There will be no sensible deflection until it reaches the position *b* in the red, corresponding to a wave-length of 0$^{\mu}$·7068, and in the prism to an angle of 40° 33′ nearly; for there is no sensible heat except in the successive images of slit S₂ formed by the prism P in the line P B. Passing further toward B we come into the heat in *c*, and next to the heat in *d*, which is less than $\frac{1}{100}$ that in the direct prismatic image, when no grating is employed.

This was the utmost limit of our power of measurement in 1883, beyond this point radiations from the grating being then absolutely insensible, and the radiation at the point *d* itself being excessively minute, even in the solar spectrum, where the heat, so far as any is found, is as a rule far greater than that in the spectrum of the arc. Accordingly I have else-where observed that these measures could be carried on as well by a large electric arc as by the sun ; but in fact, owing to the difficulties attendant on bringing the arc, which must be of immense heat, close to slit S₁, and to other causes, the sun-light would be preferable wherever it could be used.

Our observation of June 7, 1882, gave the value of the index of refraction corresponding to $\lambda = 2^{\mu}$·356, which was the lowest possibly attainable by our then apparatus. Inces-sant practise and study, resulting in improvements already referred to, have enabled us finally to measure down to a wave-length of $9 \times \lambda D_2$, corresponding to a position much below *f*. We may add that in doing so, it is sometimes con-venient to employ a bolometer wide enough to overlap the images in the other adjacent spectra of the higher orders, which we may usually do without confusing them, owing to their feebleness compared with that of the first spectrum in which we are searching.

We usually, however, employ a bolometer of not more than 1 millim. aperture, and this demands excessive delicacy in the heat-measuring apparatus, since the heat here is, approxi-mately speaking, about $\frac{1}{10,000}$ of that in the region between the sodium-lines in the direct spectrum of a rock-salt prism.

Errata.

Page 158, line 4, *after* amperes *add* through the coils of 20 ohms resistance.
—   163, — 20, *for* 39° 15′ 7″ *read* 39° 15′·7, and so to end of column.
—   168, — 8, *for* +2 *read* +Q.

This is near the limit of our present measuring-powers with the grating, even when every possible device is used to increase the extremely feeble heat in this part of the spectrum.

We commenced by using an electric arc with carbons 12 millim. in diameter in the position indicated. These were supplied by an engine of three-horse power ; but even in this case the pit of the crater did not nearly cover the very short slit (its length is 8 millim.). For these last and most difficult measurements we have been obliged to procure the use of an engine of twelve-horse power and carbons 25 millim. in diameter. With this enormous current the hottest part is not easily maintained in place. To keep it directly in front of the slit we have tried various plans, such as boring out the carbons lengthwise so as to form hollow cylinders of them, and filling the core with a very pure carbon tempered to the requisite solidity. Ordinarily it will be sufficient, however, to first form the central crater by a drill. This gives us a persistent crater, whose light, in the position shown in the engraving, filled a slit whose vertical height is 8 millim. It is probably the intensest artificial heat ever subjected to analysis.

BOLOMETER.

The changes in the bolometer since it was first described ('Proceedings American Academy,' 1881) are superficial rather than radical, and refer chiefly to the form of the case, and facilities for its accurate pointing. The linear bolometer is now made to expose to the radiant heat a vertical tape or wire of platinum, iron, or carbon. This is usually about 10 millim. long and only from $\frac{1}{1000}$ to $\frac{1}{100}$ millim. thick; but according to its special purpose it is made from 1 millim. to 0·04 millim. wide. In the latter case it appears like the vertical strand of an ordinary reticule in the focus of a positive eyepiece attached to the case, and is movable by a micrometer-screw. It is in fact in appearance a micrometer-thread, controlled in the usual way, but which is connected with the galvanometer and endowed with the power of feeling the radiations, visible or invisible, from any object to which it is directed. For very feeble sources of heat, such as those with which we are here concerned, the strip is made as much as a millimeter in width, and is not provided with a micrometer-screw, but moves with the arm carrying it, and its positions are read by the divided circle of the spectrometer to 10″ (ten seconds of arc). It is this simple form of the instrument which has been used in the present investigation, and which is shown in Plate IV. fig. 2. The bolometer is shown in position in the middle of the case, where its central strip

is accurately self-centred in the cylinder. For protection from air-currents, since the obscure heat studied will be stopped by a glass cover, we must make use of the special device I have described in the memoir just referred to, of successive chambers or drums separated by diaphragms with a common central aperture.

With these precautions, and with the special adjuncts before described, a bolometer with a strip $\frac{1}{25}$ millim. wide can be set by the invisible heat alone to within $10''$ of arc, while in the ordinary use of the linear thermopile we are liable to errors of a considerable fraction of a degree. Even with a bolometer 1 millim. wide, it will be subsequently seen, we can set to one minute of arc. This refers only to the precision of pointing attainable ; we will consider the sensitiveness of the instrument later in connection with the galvanometer.

Galvanometer.

It must be remembered that while the nominal sensitiveness of a galvanometer can be increased to any extent by increasing the astaticism of the needle (quite as nominal power can be multiplied to any extent on a telescope by altering lenses at the eyepiece), that the real or working capacity depends upon the ability to always obtain a like result under given conditions. Accordingly we have continued to devote great pains to extend our original conception, so as to make the galvanometer, as well as the bolometer, not merely an indicator of heat, but a real " meter," which shall distinctly answer the question "how much?" as to almost infinitely minute amounts of energy.

For the benefit of any physicists who may desire to repeat these experiments, we may observe that we have found the bolometer capable of almost unlimited delicacy of perception of heat, but that our chief trouble has arisen from the difficulty of constructing a galvanometer suitable to develop its full capacity for exact measurement. We have been unable to find among galvanometers ordinarily constructed one capable of indicating with precision changes in the amount of current of much less than $\frac{1}{1,000,000}$ of an ampere. It was in the construction of a galvanometer designed to measure the heat in the spectrum of the moon, that we acquired the experience which we have utilized in the present researches.

A reflecting-galvanometer of the form devised by Sir William Thomson has been used for the basis of our construction and altered as follows. (For several of the changes described I am indebted for suggestions due to the great

kindness of Sir William Thomson and Professor Rowland.) First, the short suspending fibre supplied by the makers has been replaced by one 33 centim. in length, stretched and prepared with particular care. Next, since the effect of a given minute change of current is proportionable (other things being equal) to the magnetic moment, and to the minuteness with which the angular deflection of the needle can be read, we have reconstructed the mirror and needles as follows :— For the magnets * soft sheet steel $\frac{1}{20}$ of a millim. thick is rolled up into minute hollow cylinders, each about 8 millim. long and about 1 millim. diameter. These are hardened and made to take a permanent charge of nearly 900 Gaussian units. Ten of these are placed behind the back of the mirror and ten below, making twenty in all. The reflecting mirror is accurately concave, being specially worked for the purpose, 9·5 millim. in diameter, 1 metre radius of curvature, weighing 63 milligrammes, and platinized on the front face by the discharge *in vacuo* of platinum electrodes, by the process of Prof. Wright†, of Yale College. The stem which unites the upper and lower system of the magnets is a hairlike and hollow tube of glass, while it occurred to me to replace the aluminum vane of the ordinary instruments by the wings of a dragon-fly (*Libellula*), in which nature offers a model of lightness and rigidity quite inimitable by art.

The glass plate which encloses the front of the galvanometer has optically plane and parallel sides, and the screen, placed at 1 metre distance from the mirror, is a portion of a cylinder 1 metre in radius, divided into 500 divisions of 1 millim. each. The optical arrangements for illuminating and forming an image of the wire form one of such precision that a motion of $\frac{1}{10}$ of one of these divisions can be distinctly noted. There is an independent provision, by means of which the image of a second opaque and inverted scale can be viewed by the observer through a telescope, not, as in the ordinary construction, directed onto a flat attached to the needles, but in which the concave mirror, already described, becomes the mirror of a Herschelian telescope itself. Ordinarily the condition of astaticism of the needle is such that, without any damping-magnet, it will execute a single vibration in not less than 15 nor more than 30 seconds. Much greater sensitiveness can be given to it, of course, but without, as we have found, corresponding advantage.

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* The design and construction of the hollow magnets is due to Mr. F. W. Very of this Observatory.

† Prof. Wright has had the goodness to platinize these delicate mirrors for us himself.

For the purpose of forming an estimate of the sensitiveness of the instrument it may be stated that, when making a single vibration in 20 seconds, a deflection of 1 millim. division of the scale is given by a current of 0·000,000,000,5 amperes ; and, as we have just remarked, a tenth of one of these divisions can be discriminated. That this degree of sensitiveness is associated with a real, and not nominal corresponding degree of accuracy, is shown by the fact that many series of accordant measurements have been made when the maximum deflection did not exceed three such divisions ; and that similar measures have been made in the invisible spectrum given by ice melting in a dark room, when the maximum deflection observed was 1·6 millim., and most deflections less than one millimetre. On the other hand the exposure of the same bolometer to ordinary direct sunlight with only $\frac{1}{1000}$ of the current passing, *i. e.* with the galvanometer shunted 1000 times, would drive the needle immediately, and with violence, off the scale.

Our experience has shown us that this galvanometer, in conjunction with such a bolometer as we have described, is capable of recording a disturbance of rather less than $\frac{1}{1,000,000,000}$ part. To attain corresponding accuracy in gravity determinations we should need to have a balance capable of weighing a kilogramme, which would give at the same time an unequivocal deflection for a difference of one one-thousandth of a milligramme in either pan. A deflection of 1 millim. corresponds, in the case of such a bolometer as we have used in the lunar spectrum, or in that of melting ice, to a change of temperature in the bolometer-strips considerably less than $\frac{1}{100,000}$ of a degree Centigrade, and we have just seen that about $\frac{1}{10}$ of this can be shown. In other words, about one one-millionth of a degree can be indicated by it, and a quantity less than one one-hundred-thousandth of a degree, not only indicated, but measured. It will be obvious to the practised observer that this degree of accuracy will not be in reality reached unless the bolometer-strips are perfectly protected from all extraneous radiations and air-currents, and especially unless the image is fixed upon the scale when the bolometer is not exposed to heat. This degree of precision we believe ourselves to have actually obtained.

GRATINGS.

Of the concave gratings we have three, of the very largest size. These magnificent instruments we owe, not only to the skill, but to the special kindness of Prof. Rowland, who has

been good enough to execute them for us of the very short focus and open ruling necessary for our particular work.

Let us designate them as grating No. 1, No. 2, and No. 3. The dimensions of grating No. 1 have been given in a prior memoir, but we repeat them here with some other data, for the reader's convenience. The limit of precision imposed by the use of the bolometer makes it superfluous to introduce any temperature-correction, or to give the figures with more exactness than we here do.

	Grating No. 1.	Grating No. 2.	Grating No. 3.
Radius of curvature......................	1626 mm.	1753 mm.	1627 mm.
No. of lines to millim....................	142·1	142·1	113·7
Height of ruled portion	102 mm.	80 mm.	75 mm.
Width ,, ,, ,, ...	146	132	133
Distance corresponding to 10,000 of Ångström's units on the line of wave-lengths $S_1 S_2$	231·0	249·1	185·0

The ruled portion of each of these truly superb instruments occupies from 100 to 150 centim. On their exquisite definition we need not enlarge, since sufficient of them are now in the hands of physicists to make our commendation superfluous.

We have already described the action of the grating. The essential feature, for our purpose is that, under the stated conditions, we can in theory be absolutely sure of the wavelength of the *invisible* ray under examination by choosing it a multiple of the wave-length of some *visible* line in the superposed spectrum which is coincident with slit S_2.

Thus in the case of our illustration, we have supposed the sodium-line to be used, since this is conspicuous in that of the sun and easily reproduced in that of the arc. The wavelength we are in search of is always a times the wave-length of D_2 (a being some aliquot number). In practice we thus for greater certainty always form the image of some line in the visible spectrum on slit S_2, although, as already explained, its mere position on the line $S_1 S_2$ is, if the apparatus be in adjustment, a guarantee that none but the exact ray and its multiples come under examination.

Lenses and Prisms.

The rock-salt lenses $L_1 L_2$ are of different focal lengths for different occasions. For the extremely feeble heat we are considering, we are using very clear and perfectly figured

salt-lenses of 75 millim. aperture and 350 millim. focus; this
small ratio of aperture to focus in the lenses being required
to economise the feeble heat as much as possible. The prism
used with them is first set to minimum deviation on some
visible line, and then automatically kept there for the invisible
ray under consideration.

We owe these specimens of rock-salt to the particular kind-
ness of Prof. Hastings, of Yale College, and their extremely
exact surfaces to Mr. Brashear, of Allegheny, the maker of
the surfaces on which the Rowland gratings are ruled. On
this portion of the apparatus alone very great labour has been
expended.

We have procured, through Mr. Brashear's skill, by means
previously described, a rock-salt prism having a field filled
with Fraunhofer lines, and showing distinctly the nickel line
between the D's. This is when first polished as it comes
from the maker's hands, but owing to the deliquescent nature
of the material, with the utmost care, the surfaces rapidly
deteriorate. As it is necessary for the precision of these re-
searches to determine the refracting angle of the prism, and
also the indices of refraction of some of the principal lines in
its visible and invisible spectrum, with a high degree of
accuracy, and as all these labours have to be repeated when
the prism is repolished, some idea of the labour in this portion
alone may be understood, when it is stated that the prism
has been sent to the maker and entirely refigured, and its
principal constants redetermined by us no less than thirteen
times in the past fourteen months, or since the 1st of January
1885 *.

We now give two examples of actual measurement of wave-
lengths; the first, that in sunlight in a flint prism, which we
have designated as No. 2; the second in the rock-salt prism
just mentioned.

First example of Measurement. With Flint-glass prism.
(Extract from original record.)

Station, Allegheny.
Date, March 3rd, 1886.
Temperature of apparatus $= 1°·8$ Cent.
State of sky, clear overhead, cirrus clouds near the horizon.
Aperture of slit $S_1 = 2$ millim.
      ,,   ,, $S_2 = 1$ millim.
Lenses of glass (non achromatic), focal length $= 800$ millim.
   for visible rays.

* For a full description of the constants of this prism see this Journal
for May 1886.

Grating, Rowland No. 1 (concave).

Prism, H No. 2 (glass).

Refracting angle $= 62° \ 15' \ 03''$.

Spectrum thrown east.

Galvanometer, No. 3, damping magnet at 40 centim.

Time of single vibration $= 21$ sec. (with current passing).

Bolometer, No. 16 (aperture $= 1$ millim.).

Reading on slit (before mounting prism), $0° \ 00' \ 00''$.

     ,,   ,, D_2 (through prism), $52° \ 52' \ 40''$.

Current of battery $= 0·036$ ampere*.

Reader at circle, F. W. V.

     ,,   galvanometer, S. P. L.

Object $=$ measurement of deviation of $\lambda = 4 \times \lambda D_2$ in the spectrum of the flint-glass prism H No. 2, with sunlight.

Galvanometer-deflection with arms in line (from the combined effect of all the spectra falling on slit 2), a little over 300 div.

Prismatic deviation	$49° \ 00'$	$48° \ 58'$	$48° \ 56'$	$48° \ 54'$	$48° \ 52'$	$48° \ 50'$
Galvanometer-deflections. First series	11	10	11	18	19	12
	2	13	12	15	13	15
Mean deflections	6·5	11·5	11·5	16·5	16	13·5
Galvanometer-deflections. Second series	13	21	26	32	25	20
	14	22	24	31	30	13
Mean deflections	13·5	21·5	25	31·5	27·5	16·5

Concluded maximum at $48° \ 54'$.

Making the galvanometer-deflections ordinates, and the prismatic deviations abscissæ, a smooth curve through the points of observation gives in the first case a maximum at $48° \ 54'$. The image of the slit has a certain size, and so has the bolometer-strip. The latter feels the heat before the centre of strip and image coincide ; and it is this point of the coincidence of centres which gives the maximum as denoted by the above figures.

* It must be understood that this is the *total* current of the battery. The differential current which passes through the galvanometer is of an altogether different order of magnitude, in this case probably not exceeding 0·000,000,01 ampere.

We now make a Second Series, and though the two series follow each other at a brief interval on a day described as " clear," the values of the deflections in the second series on the same points indicate nearly twice the heat in the first. The change is due to the altered diathermancy of the apparently clear sky in the brief interval. It is one of the difficulties already signalized by the writer and by others *, and is to be eliminated only by repeated observation. The second series, however, gives the same value as the first, and hence we conclude (as far as this day's observation goes) that this was the index of refraction, for the wave-length in question and for a certain flint prism. Such observations must generally be repeated on many days before a reliable result is reached; and in the case of the glass prism, which grows rapidly athermanous just beyond the limit of the solar spectrum, they are limited by the nature of the substance to little more than the wave-length in question.

We now consider, as our second example, a measurement at $5 \times \lambda D_2$, where glass can hardly be used, and where its place is supplied by the rock-salt prism, and that of the sun by the arc.

Second example of Measurement. With Rock-salt Prism.

(Extract from original record.)

Station, Allegheny.

Date, April 15th, 1886 (P.M.).

Wet bulb at 4^h 30^m (inside dark room artificially heated), $= 21°$ C.

Dry bulb at 4^h 30^m (inside dark room artificially heated), $= 26°·4$ C.

Aperture of slit $S_1 = 4$ millim.

    ,,   ,, $S_2 = 1·25$,,

Prism used, Hastings No. 1 (rock-salt).

Lenses used, focus $= 760$ millim. for visible rays.

Grating used, Rowland No. 2.

Galvanometer used, No. 3, damping magnet at 40 centim.

Time of single vibration $= 17·5$ sec. (with current passing).

Bolometer, No. 16 (aperture 1 millim.).

Setting on slit $= 0°$ $00'$ $00''$.

    ,, D_2 (spectrum thrown east) $= 41°$ $03'$ $00''$.

    ,, D_2 (,,   ,,   west) $= 39$ 57   40.

Current of battery $= 0·037$ ampere.

Reader at circle, J. P.

    ,,   galvanometer, F. W. V.

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* See Crova, *Comptes Rendus*, tome ci. p. 418 (August 10, 1885).

Arc managed by A.

Object = determination with the great arc, of the deviation in a rock-salt prism corresponding to a wave-length of $5 \times \lambda D_2 = 2^{\mu}\cdot945$.

SPECTRUM THROWN EAST.			
Deviation......	39° 21′.	39° 18′.	39° 15′.	39° 12′.
1st series ...	1·3 1·3 } 1·3	3·3 3·4 } 3·4	5·5 5·4 } 5·5	2·3 3·4 } 2·9
2nd series ...	−1·0 −0·8 } 0·9	0·6 1·6 } 1·1	7·5 6·3 } 6·9	1·7 1·7 } 1·7

SPECTRUM THROWN WEST.			
3rd series ...	1·0 0·4 } 0·7	2·1 2·7 } 2·4	4·1 3·9 } 4·0	0·2 −0·1 } 0·1
4th series ...	−0·1 0·2 } 0·1	2·3 2·4 } 2·4	3·4 3·6 } 3·5	−0·1 0·3 } 0·1

The sodium-line of the 5th spectrum fell exactly upon the slit S_2 at the beginning and end of the observation.

From smooth curves the following positions were deduced for the maximum :—

	°	′	″
Spectrum east	39	15	7
,, ,, ...	39	15	0
Spectrum west	39	16	0
,, ,, ...	39	16	4
Mean	39	15	8

The preceding examples will sufficiently indicate the means used in making the following measurements of wave-length with the grating, which have been carried on continuously from December 1885 to April 1886. As a rule, each of the sixty-two determinations in the following Table represents one or more days' labour, though in some cases two or more determinations have been secured in the same day.

M 2

They have been taken under the following conditions:—

(1) In the case even of the very lowest wave-lengths in the feeblest heat we have been able to use a linear bolometer of not more than 1 millim. width.

(2) About an equal number of observations were intended to have been taken with the prism placed so as to throw the spectrum east and west. In doing this a minute systematic error, amounting, at the greatest, to less than 1′ of arc, was found to be caused by flexure of the arm, due to the weight of the bolometer cable, and a correction for this has been applied. Otherwise the observations are given as originally made ; and as the " probable error " includes all the more or less systematic differences, due to the use of different gratings and different positions of the apparatus, it may be considered to be in this case a fair indication of the amount of error to be actually expected.

We do not know of any determination of the change produced in the refractive power of a rock-salt prism by varying temperature. A rough comparison of the deviations of Fraunhofer lines, incidentally measured in the progress of the work at different seasons, during which the temperature has varied nearly 30° C., together with the results of a single day's measures at temperatures differing by 17° C., have concurred in indicating a diminution in the deviations throughout the visible spectrum of about 11″ for a rise of temperature of 1° C.

We do not doubt that a temperature-correction is also required for the invisible spectrum; but not having yet been able to satisfactorily determine any, we think it best to leave all the observations as they stand, uncorrected for temperature, and offer them under this reserve now, with the intention of returning to them hereafter.

In the following Table we repeat, for convenience, the results of certain optical measures made on September 14th, 1885, by Mr. J. E. Keeler with the rock-salt prism, whose refracting-angle was on that date 59° 57′ 54″, and which we have reduced to 60° by the formula given in this Journal for May 1886. The wave-lengths are those due to Peirce's and Rowland's corrections of Ångström, with which we have been obliged by the authors before their formal publication.

In the succeeding portion of the Table we have the results of measures made with the bolometer in the invisible spectrum. The first column gives the source of heat; the second the wave-length selected for measurement; the third the grating employed; the fourth the refracting-angle of the prism on the day of observation; the fifth the temperature

of the apparatus; the sixth the observed deviation; the seventh the mean deviation corrected for flexure error and reduced to a refracting-angle of 60°; the eighth the resulting index of refraction for rock-salt.

Source of Heat	Wave-Length	Grating	Refracting-Angle	Temperature	Observed Deviation	Mean Deviation, reduced to 60°.	Index of Refraction.
Sun ...	μ 0·39687		59° 57′ 54″	24°	43° 14′ 44″	43° 17′ 15″	1·56833
	48614		,,	,,	41 51 47	41 54 09	1·55323
	51838		,,	,,	41 32 52	41 35 13	1·54975
	58901		,,	,,	41 02 41	41 05 00	1·54418
	58961		,,	,,	41 02 29	41 04 48	1·54414
	65630		,,	,,	40 42 56	40 45 13	1·54051
	75940		,,	,,	40 22 25	40 24 41	1·53670
Arc......	$2\times\lambda\mathrm{D}_2=$ 1·1780	1	59 57 20	24	39 46 0	} 39 49 18 ±12″	1·5301 ±·0001
			59 58 50	26	46 40 / 48 20		
Arc......	$3\times\lambda\mathrm{D}_2=$ 1·7670	1	59 57 20	24	39 31 0	} 39 34 06 ±12″	1·5272 ±·0001
			59 58 50	26	32 0 / 32 30		
Arc......	$4\times\lambda\mathrm{D}_2=$ 2·3560	1	59 58 50	26	39 24 0 / 22 40	} 39 24 18 ±18″	1·5254 ±·0001
Sun ...	....	...	59 59 34	1·1 / 0·6 / 0·6	22 24 / 24 06 / 25 06		
Arc......	$5\times\lambda\mathrm{D}_2=$ 2·9451	1	59 58 20	19	39 14 42	} 39 18 12 ±2″·4	1·5243 ±·0000
			59 57 45	21·8	15 03		
			,,	,,	13 48		
			,,	,.	16 30		
			,.	,,	15 18		
			,·	,,	17 42		
			,,	,,	17 24		
			,,	,,	15 36		
			,,	23·8	16 00		
			,,	,,	16 18		
			,,	,,	15 54		
		2	,,	26·4	15 48		
			,,	,,	16 30		
			,,	,,	15 48		
			,,	,,	15 12		
			,,	,,	15 36		
Arc......	$6\times\lambda\mathrm{D}_2=$ 3·5341	1	59 58 20	14	39 04 00	} 39 09 54 ±30″	1·5227 ±·0002
			59 57 45	16	06 48		
			,,	15	06 24		
			,.	,,	08 54		
			,,	,,	10 00		
			,·	,,	9 30		
			,,	,,	8 30		

Table (*continued*).

Source of Heat.	Wave-Length.	Grating.	Refracting-angle.			Temperature.	Observed Deviation.			Mean Deviation, reduced to 60°.	Index of Refraction.
			°	′	″	°	°	′	″	° ′ ″	
Arc......	$7 \times \lambda D_2 =$ 4·1231	1	59	57	45	15	39	02	18		
				,,		,,		01	48		
				,,		,,		00	30		
				,,		,,		02	00	39 03 42	1·5215
				,,		23·8		01	36	±12″	±·0001
				,,		,,		00	48		
				,,		,,		01	42		
				,,		,,		00	42		
Arc......	$8 \times \lambda D_2 =$ 4·7121	1	59	57	45	15	38	51	18		
				,,		,,		50	48		
				,,		,,		52	12		
				,,		,,		54	42	38 56 06	1·5201
				,,		,,		54	42	±30″	±·0002
				,,		,,		54	12		
				,,		,,		57	36		
				,,		,,		55	18		
Arc......	$9 \times \lambda D_2 =$ 5·3011	1	59	57	45	15·4	38	45	12		
				,,		,,		45	42		
				,,		,,		47	12	38 48 06	1·5186
				,,		,,		48	48	±30″	±·0002
				,,		,,		47	18		

In the following brief Table we have summarized the results of all this labour. Our working method gave the index in terms of the wave-length ; but since ordinarily the former is the known and the latter the unknown quantity, we here give the mean probable error as finally corrected as a function of the latter.

Given Indices of Refraction in Rock-salt Prism.	Wave-lengths from Direct Observation. (*a*) by the eye ; (*b*) by the bolometer.
1·5442	$\lambda D_2 = 0·5890 \pm 0·000$ (*a*)
1·5301	$2 \times \lambda D_2 = 1·1780 \pm 0·002$ (*b*)
1·5272	$3 \times \lambda D_2 = 1·7670 \pm 0·005$,,
1·5254	$4 \times \lambda D_2 = 2·3560 + 0·009$,,
1·5243	$5 \times \lambda D_2 = 2·9451 \mp 0·013$,,
1·5227	$6 \times \lambda D_2 = 3·5341 \pm 0·019$,,
1·5215	$7 \times \lambda D_2 = 4·1231 + 0·029$,,
1·5201	$8 \times \lambda D_2 = 4·7121 \mp 0·043$,,
1·5186	$9 \times \lambda D_2 = 5·3011 \mp 0·065$,,

In Plate VI. we have graphically constructed the relations between n and λ for the rock-salt prism, as far as the above wave-length of $5^\mu \cdot 3011$ or $0 \cdot 0053$ millim. The ordinates are proportional to the indices of refraction given on the axis of Y, the abscissæ to the wave-lengths on the axis of X. The two vertical dotted lines carry the eye down to the corresponding portion of the spectrum, which is visible. Between these lines lie the points of the visible spectrum observed, and the dotted curves show the results of extrapolations by various formulæ.

The actual points settled by observation are certain multiples of the wave-length D_2 ($0 \cdot 0005890$ millim.), and a small circle whose diameter equals a unit in the *third* decimal place of the scale of ordinates (indices) gives the position fixed by observation, while the distance from the centre at which the smooth curve cuts the little circle furnishes a graphic representation of its difference from observation. The labours of the past year, then, have enabled us to absolutely and directly measure the index of refraction of rays whose wave-lengths are greater than $0 \cdot 005$ millim., or, more exactly, which reach 53011 of Ångström's scale ; and to do so with an error which is probably in most cases confined to the fourth decimal place of the index. As we shall see more clearly by Plate VI., the relation between n and λ has changed from that apparently complex one we see in the visible spectrum, so that n becomes almost a simple linear function of λ, and the results of extrapolation grow to a higher order of trustworthiness than when made from points in the visible spectrum alone.

It appears to us that no formula of dispersion with which we aie acquainted [*] gives entirely correct results on extrapolation, but that among the best are Briot's and Wüllner's. We have computed the wave-lengths corresponding to indices of refraction from observed deviations in the visible spectrum according to these formulæ. The curve from Cauchy's formula we do not give, because (at least when not more than three terms are taken from observations in the visible part of the spectrum) its results are here of little value, since it declares all the radiations we are now actually dealing with to be impossible of discrimination at all. Redtenbacher's formula we have also shown in a previous memoir to be scarcely worth further consideration. The graphically constructed values are obtained by applying the formula of Briot,

[*] That proposed by Ketteler has come to the writer's knowledge too late for trial here.

$$\left(\frac{1}{n^2} = a + b\frac{n^2}{\lambda^2} + c\frac{n^4}{\lambda^4} + k\frac{\lambda^2}{n^2}\right),$$

to the four points

$$A(\lambda = 0^{\mu}\cdot7601, \; n = 1\cdot53670),$$
$$D_2(\lambda = 0^{\mu}\cdot5889, \; n = 1\cdot54418),$$
$$b_1(\lambda = 0^{\mu}\cdot5183, \; n = 1\cdot54975),$$
$$H_1(\lambda = 0^{\mu}\cdot3968, \; n = 1\cdot56833);$$

and the formula of Wüllner,

$$\left(n^2 - 1 = -P\lambda^2 + 2\frac{\lambda^4}{\lambda^2 - \lambda^2_m}\right),$$

to the three points

$$A(\lambda = 0^{\mu}\cdot7601, \; n = 1\cdot53670),$$
$$b_1(\lambda = 0^{\mu}\cdot5183, \; n = 1\cdot54975),$$
$$H_1(\lambda = 0^{\mu}\cdot3968, \; n = 1\cdot56833).$$

All these are in the visible spectrum, and from them the constants a, b, c, k, P, &c. are determined. With their aid we next inquire by the formula what wave-lengths correspond to certain given indices, and the resultant values in the infrared are then plotted from these computations. It is, however, only just to observe that the wide departure from observation here shown is by no means to be wholly attributed to error in the formula ; for minute errors of measurement, such as are always present even in the observations in the visible spectrum, are immensely exaggerated by extending the curve through extrapolation. Wüllner's formula, for instance, would give a line closely coincident with our curve in the infra-red if we took all our points for computation from that part of the spectrum. A similar remark may be made of Briot's equations, whose actual tracing, however, with the constants we obtain from the visible spectrum, shows that beyond a certain point the curve, which is its geometrical representation, from being concave to the axis of X, becomes convex, so that the relation between n and λ would, according to it, be represented by a sinuous line ; and this is not so within the limits of these observations. Its fair agreement with observation, then, within the limits of the visible spectrum and the upper part of the solar infra-red are all that can be claimed for it. Our conclusion is that all theories of dispersion known to us prove inadequate to predict the relation between wave-length and refraction. The actual relation from direct investigation is here given for the first time from the observations of the past year, which, it will be seen, thus

confirm and greatly extend the results of 1882 and 1884.
Their most salient feature is still perhaps that already noted,
i. e. while the curvature, as far as we can follow it, grows less
and less, at the last point at which we can view it the curve
is not only all but sensibly a straight line, but one making a
very definite *angle* with the axis of X. This obviously means
that beyond this point n is nearly a linear function of λ, or
that the simple equation $n = a\lambda$ would very closely represent
this portion of the curve. It means also that, as far as these
observations extend, we find scarcely any limit to the index
of the ray which the prism can transmit except from its own
absorption. I do not, it will be observed, undertake to
advance without limit beyond observation, or to discuss what
would happen with wave-lengths so great that the index be-
came 0 or negative, as it would with an indefinite prolonga-
tion of the curve, if its direction remain unaltered. An
intelligible physical meaning might perhaps be attached to
these cases ; but I here confine myself to the results of
direct observation and to the now established fact that the
increase of the crowding together of the rays at the red end,
which is so conspicuous a feature in the upper prismatic
spectrum, has almost wholly ceased, and that the dispersion
has become approximately uniform, the action of the prism
here being assimilated to that of the diffraction-grating itself.
I shall not venture to treat of the theoretical import of this,
further than to remark that the ordinary interpretation of
Cauchy's theory will apparently lead us to conclude that dis-
persion must sensibly cease at the point where the wave is so
long that the size of the components of matter is negligible
in comparison. In other theories, also, there appears to be a
point below which the index of refraction should never fall ;
and we might anticipate that the curve would accordingly
tend to become parallel to the axis of X. Of course we
cannot assert from observation that it will not finally do so ;
but within the very extended limits in which we have fol-
lowed it the contrary happens, and the curve presents a
continuously increasing angle with that axis.

These results, then, in some material points are in contra-
diction to what has usually hitherto been believed [*].

Let me repeat that one consequence of the fact that the

[*] I am very desirous that they should be verified by physicists, and I
have therefore given particulars in some detail of my methods and appa-
ratus here. I have requested the skilful artists (Mr. William Grunow,
Mechanician to the U. S. Military Academy, West Point, N.Y., and Mr.
J. A. Brashear, Allegheny, Penn.) who have so successfully constructed
this apparatus to place at the command of physicists all or any details
of it.

curve is approaching a straight line is that, unless there is some immediate change in its character, such as we have no right to expect, extrapolation considerably beyond the point to which we have measured will be comparatively easy and safe. I am aware of the danger attending all extrapolation, but I must insist upon the fact that the old ones, which we have seen falsified by experiment, rested on an extremely limited region of the curve, that namely for the visible region of the spectrum, in which the relation between n and λ is also wholly different; while those on which we now briefly enter depend upon far greater material for induction (about eight times that included in the visible spectrum), which we can also use under much more favourable conditions.

Since the curve still presents a slight convexity to the axis of abscissæ, unless its character changes in a way which we have no ground to expect, a tangent at any point will meet that axis sooner than the curve itself will. Accordingly, if we now ask what wave-length corresponds to any point in the hitherto unexplored region, for instance the maximum in the spectrum of boiling water, whose index * for the 60° rock-salt prism is 1·5145, or that of melting ice, whose index is 1·5048, we can answer as follows : first, this unknown wave-length is at any rate greater than $5^{\mu}\cdot3$, since to this point we have investigated by direct measurement; second, since the tangent to our curve, even at the point 5^{μ}, meets the line corresponding to the index of the maximum heat in boiling water at over 7^{μ}, and a line corresponding to the maximum ordinate in the spectrum of melting ice at over 10 , and since the curve without some change in its essential character cannot meet these lines, save at still greater wave-lengths, it follows that the wave-length of the maximum of the spectrum from boiling water is probably at least ·0075 millim., and that of the maximum in the spectrum from melting ice is over 0·01 millim. In an article in the *Comptes Rendus* of the Institute of France, Jan. 18, 1886, and in preliminary memoirs, we deferred giving the actual values, but gave (explicitly as minimum values which we believed much within the truth) 5^{μ} to 6^{μ}. That our caution led us to understate the, even then, most probable value, may be seen from the statements just made, which are founded on still later observations.

As we proceed further out, extrapolation becomes, of course, more untrustworthy. We can only say that if the curve maintains its present inclination to the axis of X, the wave-lengths of the extreme radiations recognized in the rock-salt prism must indefinitely exceed 0·03 millim.

* See this Journal for May 1886, plate iv. fig. 3.

We have shown that the various complex formulæ founded
on theoretical considerations differ from observation ; and as
we have remarked, they have the minor objection also of
being extremely difficult of application to practical uses,
owing to the inordinately tedious numerical computations
involved where many places are to be calculated.

Struck by the resemblance of the actual curve of observa-
tion, as viewed in a large graphical construction, to a hyper-
bola, I was therefore led some years ago to use the equation
of the hyperbola as an empirical one for interpolations in the
infra-red without attaching any physical meaning to it. The
further the investigation has been pushed in that part of the
spectrum, the more exact the resemblance has become. That
it is notable will be seen on consulting Plate VI.*, where the
hyperbola does not appear as a distinct curve, because its
variation from the smooth curve of observation cannot be
recognized on this scale.

In obtaining this we have proceeded as follows :—Having
five disposable constants, we have taken five nearly equidistant
points on the smooth curve of observation, remembering that
if the axis of Y is not exactly asymptotic to the curve thus
described, we are not necessarily to impute the difference to
a fault in the equation chosen, since the condition that the
curve of observation shall be rigorously asymptotic to this
axis can in any case only be satisfied by infinite exactness of
measurement.

It will be observed that my estimates of the extreme wave-
lengths are in no way founded on the use of this hyperbola,
and that I do not assert that it has any physical meaning.

I have elsewhere observed that, while Herschel in 1840,
Draper in 1842, Fizeau and Foucault in 1846, Lamansky in
1870, with others since, had observed bands in the infra-red
prior to 1881, yet that nothing was exactly known as to the
wave-lengths of these bands, even to those who discovered
them. It is very likely that the (probably telluric) absorp-
tion-band in the solar spectrum placed on our chart (*Comptes
Rendus,* Sept. 11, 1882) at $1^{\mu}\cdot38$ has been recognized by
more than one of the above-mentioned observers, yet so little
was known as to its actual position even a few years since,
that we find the elder Draper, in reviewing these discoveries
in 1881, and speaking with the authority of one who was
himself a discoverer, expressing his doubt as to the possibility
of any wave-length so great as $1^{\mu}\cdot08$ having really been

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* It should be mentioned that some of the observations on which the
computations are founded have been added since this drawing was pre-
pared for the engraver.

Extreme Lengths of Visible and Invisible Etherial Radiations and of Sonorous Waves.

Quality of radiations and means of recognition.	Wave-lengths in units of one miilimetre.	Description.
Invisible Ultra-violet radiations (Photography).	0·000185	Extreme rays of aluminum in the induction spark, according to Cornu. Recorded by photography.
	0·000295	Extreme limit of solar spectrum at sea-level on best days, according to Cornu. Recorded by photography.
Visible radiations (Eye).	0·00036	Limit of lavender light, visible to normal eyes.
	0·00081	Extreme limit of deep red light, visible to normal eyes.
Beginning of Infra-red (Photography).	0·0010	Supposed extreme possible limit of infra-red wave-lengths in 1881. (According to J. W. Draper.)
(Phosphorescence).	0·0015	Wave-lengths assigned by H. Becquerel to lowest absorption-band known to him in 1884.
	0·0027	Sensible limit of solar infra-red rays which penetrate our earth's atmosphere. Determined by the grating and bolometer, 1882.
Invisible Infra-red radiations from terrestrial sources (Bolometer).	0·0053	Limit of absolute measurement of wave-lengths corresponding to a given index of refraction in the case of a rock-salt prism. Determined by the Rowland grating and bolometer; Allegheny, 1886.
	0·0075	Approximate position of the maximum ordinate in the "heat" spectrum from a lamp-blacked surface at the temperature of boiling water (100°); Allegheny, 1886.
	0·011	Approximate position of the maximum ordinate in the "heat" spectrum from a lamp-blacked surface at the temperature of melting ice (0°); Allegheny, 1886.
	0·030	Approximate estimate of the minimum value assignable to the longest wave recognizable by the bolometer in the "heat" spectrum from a rock-salt prism.
Sonorous vibrations (Ear).	14	Length of shortest sound-wave corresponding to highest musical note perceptible by human ear. (Approximately 48,000 s. v. per. sec., Savant.)

observed; and M. H. Becquerel (*Annales de Chimie et de Physique*, 1883, tome 30) gives the wave-length of the longest band known to him as $1^{\mu}\cdot5$. These remarks will not be superfluous as an introduction to the preceding table, which presents a summary view of the advances made beyond the above-named point in the last five years.

Broadly speaking, we have learned, through the present measures with certainty, of wave-lengths greater than 0·005 millim., and have grounds for estimating that we have recognized radiations whose wave-length exceeds 0·03 millim.; so that while we have directly measured to nearly 8 times the wave-length known to Newton, we have probable indications of wave-lengths far greater, and the gulf between the shortest vibration of sound and the longest known vibration of the ether is now in some measure bridged over.

In closing this memoir I would add that the very considerable special expenses which have been needed to carry on such a research, have been met by the generosity of a citizen of Pittsburgh, who in this case, as in others, has been content to promote a useful end, without desiring publicity for his name.

I cannot too gratefully acknowledge my constant obligation to the aid of Mr. F. W. Very and Mr. J. E. Keeler of this Observatory, who have laboured with me throughout this long work. In the prolonged numerical and other computations rendered necessary, I have been aided by Prof. Hodgkins of Washington, and by Mr. James Page of this Observatory.

Allegheny Observatory,
    May 31, 1886.

XX. *An Extension of a Theorem of* Professor Sylvester's *relating to Matrices. By* A. BUCHHEIM, *M.A.*[*]

ONE of Prof. Sylvester's fundamental theorems in the theory of matrices is what he calls the interpolation-formula : viz. if m be a matrix of order n, and $\lambda_1 \ldots \lambda_n$ its latent roots, we have, ϕ being any function,

$$\phi m = \Sigma \frac{(m-\lambda_2)(m-\lambda_3)\ldots(m-\lambda_n)}{(\lambda_1-\lambda_2)(\lambda_1-\lambda_3)\ldots(\lambda_1-\lambda_n)} \phi\lambda_1.$$

This theorem only applies so long as the latent roots are unequal. In this note I extend it to matrices having equalities

among their latent roots.   In a paper on the theory of matrices, published in vol. xvi. of the ' Proceedings of the London Mathematical Society,' I have given a canonical form to which matrices can be reduced.   If we have a matrix m with r distinct latent roots $\lambda_1 \ldots \lambda_r$, of multiplicities $n_1 \ldots n_r$, I have shown that there are n_i *latent points* answering to λ_i, and that these can be arranged in groups, such that for one of these groups $(e_1 \ldots e_s)$ we have

$$m = \frac{\lambda_i e_1}{e_1} \quad \frac{\lambda_i e_2 + e_1 \ldots \lambda_i e_s + e_{s-1}}{e_2} \qquad \frac{}{e_s}.$$

It can be shown without difficulty that if ϕ is any function, we have

$$\phi m = \frac{\phi \lambda_i e_1}{e_1} \quad \frac{\phi \lambda_i e_2 + \phi' \lambda_i e_1 \ldots \phi \lambda_i e \; + \phi' \lambda_i e_{s-1} + \ldots + \phi^{(s-1)} \lambda_i e_1}{e_2} \qquad \frac{}{e_s}$$

Now call s the length of the *chain* $(e_1 \ldots e_s)$, and let s_i be the length of the longest chain appertaining to λ_i; then it is easy to see that the extension of Prof. Sylvester's theorem amounts to finding a function f such that

$$f^{(k)}(\lambda_i) = \phi^{(k)}(\lambda_i) \quad \begin{array}{l} i = 1 \ldots r \\ k = 0 \ldots s_i - 1 \end{array};$$

and that these conditions are satisfied if we take

$$fx = \sum_{i=1}^{r} \frac{\chi_i x}{\chi_i \lambda_i} \left[\frac{\phi x}{\chi_i x} \right]_{\lambda_i}^{s_i}$$

where

$$\chi_i x = (x - \lambda_1)^{s_1} (x - \lambda_2)^{s_2} \ldots (x - \lambda_{i-1})^{s_{i-1}} (x - \lambda_{i+1})^{s_{i+1}} \ldots (x - \lambda_r)^{s_r}$$

$$[\psi x]_\lambda^r = \psi \lambda + (x - \lambda) \psi' \lambda + \ldots + \frac{(x - \lambda)^{r-1}}{r-1 \,!} \psi^{(r-1)} \lambda;$$

and we therefore have

$$\phi m = \sum_{i=1}^{r} \frac{\chi_i m}{\chi_i \lambda_i} \left[\frac{\phi m}{\chi_i m} \right]_{\lambda_i}^{s_i}.$$

It is obvious that this reduces to Prof. Sylvester's formula if the latent roots are all unequal, since in this case s_1, s_2, &c. are all equal to unity.

The Grammar School, Manchester,
        June 25, 1886.

XXI. *Notes on Magnetism.*—I. *On the Energy of Magnetized Iron.* By Lord RAYLEIGH, *Sec. R.S.**

THE splendid achievements of the last ten years in the practical application of Magnetism have given a renewed impetus to the study of this subject which is sure to bear valuable fruit. Especially to be noted are two memoirs recently published in the ' Philosphical Transactions' of the Royal Society, by Prof. Ewing[†], and by Dr. Hopkinson[‡], in which are detailed very important data derived from laborious experiment, accompanied by much interesting and suggestive comment.

The results of observation are usually expressed, after the example of Rowland and Stoletow, in the form of curves showing the relation between $\mathfrak{B}$ and $\mathfrak{H}$, the magnetic induction and the magnetizing force. It may be well here to recall the convention in accordance with which $\mathfrak{H}$ is measured. At any point in air, the magnetic force is defined in an elementary manner, and without ambiguity, but when we wish to speak of magnetic force in iron, further explanation is needed. The continuity of the iron is supposed to be interrupted by an infinitely thin crevasse in the interior of which we imagine the measurement to be effected. If the crevasse is parallel to the direction of magnetization, the force thus found is denoted by $\mathfrak{H}$, and is independent of free magnetism on the walls of the crevasse. If, however, the crevasse be perpendicular to the lines of force, there is a full development of free magnetism ($\mathfrak{I}$) upon the walls, and the interior force is now $\mathfrak{B}$, equal to $\mathfrak{H} + 4\pi\mathfrak{I}$. In the estimation of $\mathfrak{H}$ (as well as of $\mathfrak{B}$) the influence of all free magnetism, not dependent upon the imaginary interruption of continuity, is of course to be included. On this account the value of $\mathfrak{H}$ in the interior, and even at the centre, of a bar of iron placed in an otherwise uniform magnetic field, is greatly reduced, unless the length of the bar be a very large multiple of the diameter.

Experiment shows that the relation of $\mathfrak{B}$ to $\mathfrak{H}$ is not of a determinate character. In a cycle of operations, during which $\mathfrak{H}$ is first increased, and is afterwards brought back to its original value, the induction $\mathfrak{B}$ is always greater on the descending than on the ascending course. This phenomenon, which is exemplified familiarly by the retention of magnetism in a bar after withdrawal of the magnetizing force, is

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* Communicated by the Author.
† " Experimental Researches in Magnetism," vol. clxxvi. part ii. p. 523.
‡ " Magnetization of Iron," *ibid.* p. 455.

called by Ewing *hysteresis.* The accompanying curve ABCDEFGA (fig. 1) is copied from one given by him as applicable to very soft iron, conducted round a cycle from strong negative to strong positive magnetization and back again. The " residual magnetism " or " retentiveness " (OE) amounts to a large fraction (sometimes to 93 per cent.) of the maximum.

Fig. 1.

The work spent in carrying the iron round a magnetic cycle is represented by $-\int \mathfrak{J}\, d\mathfrak{H}$, as was first shown by Warburg[*], who supposes the magnetic force operative upon the soft iron to be due to permanent magnets, and variable with their position. The work required to carry the permanent magnets through the proposed cycle of motions is then proved to have the above written value, applicable to the unit of volume of the soft iron. If $\mathfrak{J}$ were proportional to $\mathfrak{H}$, or even related to it in any determinate manner, the integral would vanish ; but on account of hysteresis it has a finite value.

So long as we limit our attention to complete cycles, we may write indifferently $-\int \mathfrak{J}\, d\mathfrak{H}$, or $-\dfrac{1}{4\pi}\int \mathfrak{B}\, d\mathfrak{H}$, since $\int \mathfrak{H}\, d\mathfrak{H}$ vanishes. Again, under the same restriction,

$$-\int \mathfrak{J}\, d\mathfrak{H} = +\int \mathfrak{H}\, d\mathfrak{J} = \frac{1}{4\pi}\int \mathfrak{H}\, d\mathfrak{B}.$$

When, however, we wish to consider incomplete cycles, especially with reference to the behaviour of soft iron, it is more suitable to take $\mathfrak{J}$ as independent variable. We are led naturally to this form if we suppose that, as in the more important practical applications, the varying magnetizing force is due to an electric current, upon which the magnetized iron reacts inductively[†].

In order to avoid the question of free polarity, we may
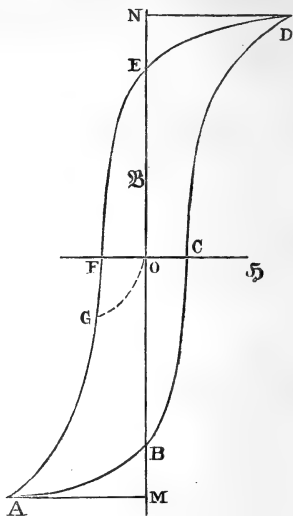
* Wied. *Ann.* xiii. p. 141 (1881).
† Hopkinson, *l. c.* p. 466.

consider, first, a ring electro-magnet with an iron core, of length l and section σ. If n be the number of windings, the whole inductive electromotive force is $n\sigma \cdot d\mathfrak{B}$, and the element of work is $n\sigma C \cdot d\mathfrak{B}$, C being the current at the moment in question. But

$$l\mathfrak{H} = 4\pi nC ;$$

so that the element of work is, per unit of volume of iron,

$$\frac{1}{4\pi} \, \mathfrak{H} \, d\mathfrak{B}.$$

If we express $\mathfrak{B}$ in terms of $\mathfrak{H}$ and $\mathfrak{J}$, we have

$$\frac{1}{4\pi} \, \mathfrak{H} \, d\mathfrak{B} = \frac{1}{4\pi} \, \mathfrak{H} \, d\mathfrak{H} + \mathfrak{H} \, d\mathfrak{J},$$

of which the latter part is specially due to the iron. In practice the former part is small, and the distinction between

$$\frac{1}{4\pi} \, \mathfrak{H} \, d\mathfrak{B} \text{ and } \mathfrak{H} \, d\mathfrak{J}$$

may often be disregarded.

But it is by no means a matter of indifference whether we take $\mathfrak{B} \, d\mathfrak{H}$ or $\mathfrak{H} \, d\mathfrak{B}$. The difference between the two modes of reckoning may be exemplified in the case of iron already nearly "saturated," and exposed to an increasing force. Here $\int\mathfrak{B} \, d\mathfrak{H}$ is large, while $\int\mathfrak{H} \, d\mathfrak{B}$ is small; so that the latter corresponds better with the changes which we suppose to be taking place in the iron, as well as to the circumstances of ordinary practice.

Let us now consider a little more closely the cycle of fig. 1. From A to B, $\mathfrak{H}$ is negative, while $d\mathfrak{B}$ is positive; so that along AB the inductive electromotive force is in aid of the current, and work is received *from* the iron of amount represented by the area ABM. From B to D, $\mathfrak{H}$ is positive as well as $d\mathfrak{B}$, and work represented by BDNB may be supposed to be put *into* the iron. From D to E, work, represented by NED, is received from the iron, and from E to A work, represented by AME, is expended. From this we see that not only is work, represented by the area ABCDEA, dissipated in the complete cycle, but that at no part of the cycle is there more than an insignificant fraction of work recovered. The case is not one of a storing of energy recoverable with a small relative loss, but rather one of almost continuous dissipation.

And here the question is forced upon us, whether it is true, as is usually supposed, that the strong residual magnetism at

E is really a store of energy. From the fact that the magnetism may be got rid of by very moderate tapping, we may infer, I admit, that some energy is necessarily dissipated in passing from the magnetized to the unmagnetized condition, but the dissipation may be exceedingly small ; and the argument is not conclusive, since the mechanical energy of the vibrations may be involved in the process. If we attempt to demagnetize the iron in a straightforward manner by the application of a reversed force, following the course indicated by EFGO, then, so far from recovering, we actually expend energy—that, namely, represented by the area EFGOE. For practical purposes, at any rate, it would seem that magnetized iron cannot be regarded as the seat of available energy.

The opposite opinion, which is widely entertained, appears to depend upon insufficient observance of the distinction, vital to this subject, between closed and unclosed magnetic circuits. It is not disputed that available energy accompanies the magnetization of a short bar of iron, but this is in virtue of the free polarity at the ends. The work stored is in fact that which might be obtained, were the bar flexible, by allowing the ends to approach one another, under their mutual attraction. When this operation is finished, so that the bar has become a ring, there is no longer any work to be got out of it, though it remains magnetized.

In further illustration of this matter, reference may be made to some interesting observations by Elphinstone and Vincent * on closed magnetic circuits. As is well known, the armature of a horseshoe electromagnet remains strongly attracted after cessation of the battery-current. If, even after a considerable interval of time, the coils of this electromagnet were connected with those of a second electromagnet also provided with an armature, and the first armature were then violently pulled away, attraction set in and persisted between the second armature and its electromagnet, the magnetism of the original circuit being as it were transferred to the second. Or, if a galvanometer were substituted for the second electromagnet, a deflection followed the forcible withdrawal of the armature. In these experiments the necessary energy is obtained, not from the magnetism of the closed circuit, but from the work done in opening it, that is in pulling away the armature.

These considerations lead me to differ from Prof. Ewing when he says † :—" In connection with ' secondary generators ' and induction-coils generally, the bearing of the first part of

* Proc. Roy. Soc. vol. xxx. p. 287 (1880).
† *l. c.* § 34, p. 554.

this paper should be noted, as showing the enormous advantage which a ring-shaped core, or core forming a complete magnetic circuit, possesses over a short bar-core with ends. In an ordinary induction-coil, so long as the current in the primary circuit is merely made and broken, a short core is necessary, since a ring-core would lose but a small percentage of its magnetism at each brake, but where reversal of the magnetizing takes place, a core approximating to the condition of endlessness has an advantage in respect of power which fig. 3 makes obvious." I confess that I do not follow this. It seems to me, on the contrary, that a closed magnetic circuit is above all things to be avoided, as leading to waste of the greater part of the power transferred.

A like objection applies to the use of a closed electro-magnet as a "throttle" in an alternate-current circuit.

When we know, as from Prof. Ewing's results, the behaviour of a given sample of iron under the influence of various forces $\mathfrak{H}$ *actually operative*, we can deduce by means of Poisson's theory the magnetism assumed by ellipsoids of any shape in response to any uniform *external* force $\mathfrak{H}'$. If $\mathfrak{I}$ be the magnetization parallel to the axis of symmetry ($2c$), the demagnetizing effect of $\mathfrak{I}$ is $N\mathfrak{I}$, where N is a numerical constant, a function of the eccentricity (e) [*]. When the ellipsoid is of the ovary or elongated form,

$$a=b= \sqrt{(1-e^2)}c,$$
$$N=4\pi\left(\frac{1}{e^2}-1\right)\left(\frac{1}{2e} \log \frac{1+e}{1-e}-1\right)$$

becoming in the limiting case of the sphere ($e=0$)

$$N=\frac{4\pi}{3};$$

and at the other extreme of elongation assuming the form

$$N=4\pi \frac{a^2}{c^2}\left(\log \frac{2c}{a}-1\right).$$

If the ellipsoid is of the planetary form,

$$a=b= \frac{c}{\sqrt{(1-e^2)}} [†],$$

and

$$N=4\pi \left(\frac{1}{e^2}- \frac{\sqrt{(1-e^2)}}{e^3} \sin^{-1}e\right).$$

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[*] Maxwell's 'Electricity and Magnetism,' § 438.
[†] There is here a slight variation from Maxwell's notation.

In the case of a very flattened planetoid ($e=1$), N becomes in the limit equal to 4π.

The force actually operative upon the iron is formed by subtracting $N\mathfrak{J}$ from that externally imposed, so that

$$\mathfrak{H} = \mathfrak{H}' - N\mathfrak{J};$$

and if from experiments on very elongated ellipsoids ($N=0$) we know the relation between $\mathfrak{H}$ and $\mathfrak{J}$, then the above equation gives us the relation between $\mathfrak{H}'$ and $\mathfrak{J}$ for any proposed ellipsoid of finite elongation. If we suppose that $\mathfrak{H}$ is plotted as a function of $\mathfrak{J}$, we have only to add in the ordinates $N\mathfrak{J}$, proper to a straight line, in order to obtain the appropriate curve for $\mathfrak{H}'$.

As an example, let us apply this method to deduce the behaviour of the soft iron of Ewing's fig. 2, when made into an ellipsoid whose polar axis is fifty times the equatorial axis, and carried round a cycle through strong positive and strong negative magnetism. We have

$$N = \frac{4\pi}{50^2} \{\log_e 100 - 1\} = 4\pi \times \cdot 001442.$$

The curve ABC (fig. 2), traced from Prof. Ewing's, gives

Fig. 2.
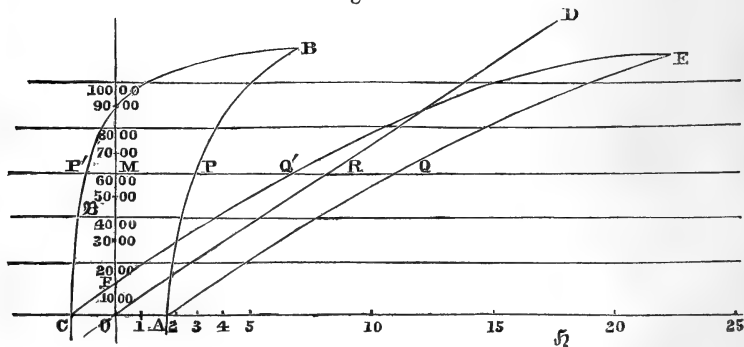
the relation between $\mathfrak{H}$ and $\mathfrak{B}$, the latter of which we may identify with $4\pi\mathfrak{J}$ *. The equation of the straight line is

$$\mathfrak{H} = N\mathfrak{J} = \cdot 001442 \times 4\pi\,\mathfrak{J};$$

and with allowance for the different scales adopted for ordinates and abscissæ, is represented on the diagram by OD.

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* The curve is symmetrical with respect to O as centre, and $\mathfrak{H}$ is measured in C.G.S. units.

In order to find the points Q, Q′ appropriate to the ellipsoid (50 : 1) from P, P′, we have merely to measure PQ, P′Q′ equal to RM. We thus obtain the curve AQEQ′FC, on which the points of zero magnetization are the same as on the original curve *. We see that a much stronger field is now required to produce the higher degrees of magnetization, and that there is less hysteresis—the magnetic state is more nearly a definite function of the external field. A similar construction might be used reversely to pass from observed results relative to ellipsoids of moderate elongation to the curve appropriate to ellipsoids of infinite elongation, on which alone we can base our views of the real character of magnetic media.

Prof. Ewing has traced by experiment the influence of various degrees of elongation on the magnetism of cylindrical rods. Results of this kind are exhibited in his fig. 3, but they are not strictly comparable with those obtained above, not only because the latter relate to ellipsoids, but also on account of the different character of the magnetic operations represented. His curves begin at a condition of zero field and zero magnetization.

The work expended in producing a small change of magnetization of the ellipsoid, acted upon by a uniform field, is $\mathfrak{H}'d\mathfrak{I}$ simply per unit of volume. This we may see, perhaps most easily, by supposing the iron to be replaced by an electric current of equal magnetic moment. The element of work done then depends upon the coefficient of mutual induction M of the two circuits, and M may be regarded as the number of lines of force due to the original current which pass through the fictitious circuit. The whole work is thus

$$\int \mathfrak{H}'d\mathfrak{I} = \int \mathfrak{H}\, d\mathfrak{I} + N\int \mathfrak{I}\, d\mathfrak{I}$$
$$= \int \mathfrak{H}\, d\mathfrak{I} + \tfrac{1}{2} N\, \mathfrak{I}^2,$$

if we reckon from the condition of zero magnetization. The first part is that already considered, and shown to be almost entirely wasted ; the second, which in most cases of open magnetic circuits is much the larger, is completely recovered when the iron is demagnetized.

Thus in fig. 2, since QQ′ = PP′, the areas of the two curves are the same, which indicates that the same amount of work is dissipated in a complete cycle. But the work absorbed during one part and restored during the remainder of the

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* Dr. Hopkinson (*loc. cit.* p. 465) has already applied this method to the determination of the particular point F, indicative of the residual magnetism in the ellipsoid, when the external force is withdrawn.

cycle is much greater in the case of AEC, corresponding to the ellipsoid of moderate elongation.

The coefficient N reaches its maximum when the ellipsoid is very oblate. In this case

$$\int \mathfrak{H}' d\mathfrak{I} = \int \mathfrak{H} \, d\mathfrak{I} + 2\pi \mathfrak{I}^2,$$

which is applicable to large plates magnetized perpendicularly to their surfaces. This is the form to which the iron must be reduced in order that a given magnetization of a given volume may store* the largest amount of energy. In this case the energy is nearly all recoverable; but we must remember that no practicable field would carry the magnetization very far.

In the theory of alternating currents the neighbourhood of of iron is often treated as if its only effect were to increase the self and mutual induction of the circuits. A writer conversant with experiment usually guards himself by a reference to the currents induced in the iron considered as a conductor. The latter effect may be in great measure eliminated by a proper subdivision of the iron, with intervening non-conducting strata; but a glance at fig. 2 shows at once that, apart altogether from internal currents, the influence of the iron is of a more complicated character. If the curve connecting $\mathfrak{I}$ (or $\mathfrak{B}$) and $\mathfrak{H}$ were a straight line, the same on the upward as on the downward course, then the presence of iron would simply increase the self-induction. When the iron constitutes a closed magnetic circuit, this is very far from being true. Indeed it would be nearer the mark to say that the iron increases the apparent *resistance* of the electric circuit, leaving the self-induction unchanged. In so far as the curve of fig. 2 can be identified with an ellipse, the reaction of the iron can be represented as equivalent to a change in the apparent resistance *and* self-induction of the circuit. Which of the two is the more important depends somewhat upon the other circumstances of the case ; but with closed electromagnets the magnetic work dissipated during the period (corresponding to increased resistance) is always greater than the work spent during one part and recovered during the remainder of the period (corresponding to increased self-induction). On this account, the resistance of an iron wire to variable currents is greater than to steady currents, even though the current be constrained to be uniformly distributed over the section. In the absence of such constraint, the resistance undergoes a further increase in consequence of the tendency of the cur-

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* It is not meant here to imply that the energy is resident *in* the iron.

rent to concentrate itself towards the exterior*. In general both causes must cooperate to produce an apparent increase of resistance to variable currents.

When the magnetic circuits are open, as with bars of iron of moderate length, the reaction of the iron manifests itself mainly as increased self-induction. This happens also in the case of closed magnetic circuits, when the magnetic changes are very small.

In general, since the curve of fig. 2 differs widely from an ellipse, the reaction of the iron cannot be fully represented as equivalent to a change in the resistance and self-induction of the magnetizing circuit. In any case of strict periodicity the reaction may, however, be analyzed, in accordance with Fourier's theorem, into harmonic components with periods which are submultiples of the original period. The neighbourhood of iron may thus introduce overtones into what would otherwise be a simple sound.

Terling Place, Witham, Essex,
July 4.

XXII. *On the Salts of Tetrethylphosphonium and their Decomposition by Heat. By* Prof. E. A. LETTS, *Ph.D., &c., Queen's College, Belfast, and* NORMAN COLLIE, *Ph.D., Science Lecturer, The Ladies' College, Cheltenham*†.

ONE of us (in conjunction with another chemist) has already pointed out the very striking analogies which exist between the elements phosphorus and sulphur and their compounds ‡, and we were anxious to continue our experiments in connection with this question.

The action of heat on methyl-sulphine compounds has been studied by Crum Brown and Blaikie §, who have shown that they decompose in a perfectly simple and definite manner. We were accordingly desirous of ascertaining whether the analogous phosphorus derivatives, namely the salts of tetrethylphosphonium, would behave similarly.

We had already investigated the action of heat on some of the salts of tetrabenzylphosphonium ‖, but the results were

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* "On the Self-induction and Resistance of Straight Conductors," Phil. Mag. vol. xxi. (1886) p. 388.

† Communicated by the Authors.

‡ Crum Brown and Letts, Trans. Roy. Soc. Edin. vol. xxviii. p. 571 ; and Letts, Trans. Roy. Soc. Edin. vol. xxviii. p. 583.

§ Crum Brown and Blaikie, *Journal f. prakt. Chemie* [2] xxiii. p. 395.

‖ Letts and Collie, Trans. Roy. Soc. Edin. vol. xxx. part i. p. 181.

not very satisfactory, owing to the tendency of the benzyl radical to split into hydrocarbons at the high temperature at which the decomposition occurred.

We thought it probable that the salts of tetrethylphosphonium would decompose at a lower temperature, and would therefore be better suited for investigation. Several of the salts of tetrethylphosphonium which we investigated had already been described by Hofmann * ; one or two, however, we prepared for the first time.

Preparation of Iodide of Tetrethylphosphonium.

As this salt was the starting-point for the preparation of the other compounds of the phosphonium, we devoted considerable care to its manufacture in the pure state.

When triethylphosphine is added to iodide of ethyl, the two combine rather suddenly and with considerable evolution of heat, so that unless care is exercised loss of material may easily occur. Hofmann suggests that the iodide of ethyl should be diluted with ether before the addition of the phosphine, and at first we acted on this suggestion; but subsequently we found that, by carefully cooling the mixture and using excess of iodide of ethyl, the reaction could be kept completely under control, and that the addition of ether was unnecessary. The first quantity of the phosphonium iodide was prepared in etherial solution : 30 grms. of triethylphosphine were added to a mixture of 43 grms. of pure iodide of ethyl and 500 grms. of ether. The mixture was left for a night, when 30 grms. of feathery white crystals separated. These were washed with dry ether, and dried *in vacuo* over sulphuric acid ; the determination of iodine in them was made volumetrically.

0·600 grm. required 21·9 cubic centim. decinormal nitrate of silver solution = 0·27813 grm. iodine = 46·36 per cent.

From the ether mother liquors a further batch of crystals was obtained.

The next quantity of the phosphonium iodide was prepared by the direct addition of 30 grms. of triethylphosphine to a large excess (200 grms.) of iodide of ethyl, which was contained in a flask, fitted with an upright condenser and placed in water. After a short time violent ebullition occurred, and a considerable quantity of iodide of ethyl volatilized into the condenser. On distilling off the excess of this from a water bath, a snow-white mass of crystals remained.

These were recrystallized by dissolving them in alcohol and precipitating with ether. A determination of iodine in the

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* Hofmann and Cahours, *Annalen*, vol. civ. p. 1.

product was made both (*a*) before, and (*b*) after recrystalliza-
tion.

(*a*) 0·4065 grm. (dried over sulphuric acid) required 14·85
cubic centim. decinormal nitrate of silver solution=0·1886
grm. iodine=46·40 per cent.

(*b*) 1·085 grm. (dried at 100° C.) required 39·6 cubic
centim. decinormal nitrate of silver solution=0·5029 grm. of
iodine=46·35 per cent.

A third quantity of the salt made in a similar manner was
also analyzed.

0·9485 grm. required 34·7 cubic centim. of decinormal
nitrate of silver solution=0·44069 grm. iodine=46·46 per
cent.

These determinations show that in each case the phospho-
nium salt was pure.

Obtained.				Calculated for
1.	2.	3.	4.	$(C_2H_5)_4PI.$
46·36	46·40	46·35	46·46	46·35 per cent. iodine.

As regards the properties of this salt, we have nothing to
add to the description given by Hofmann and Cahours [*].

Action of Heat on the Hydrate of Tetrethylphosphonium.

Hofmann and Cahours [†] had already studied the behaviour
of this compound when heated, and had found that it decom-
posed in the following manner :—

$$(C_2H_5)_4POH = (C_2H_5)_3PO + C_2H_6 ;$$
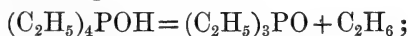
but as they did not make any exact determination of the
quantity of phosphine oxide, nor of the ethane produced, and
as we were particularly anxious to ascertain whether the
above equation expresses the whole change which occurs, we
determined to repeat the experiment.

10 grms. of pure iodide of tetrethylphosphonium were con-
verted into the hydrate by adding excess of oxide of silver to
its aqueous solution. The liquid filtered from the resulting
iodide of silver was transferred to a distilling flask, connected
with an apparatus for collecting gas, and heated. At first no
change occurred except simple ebullition ; but when the solu-
tion grew concentrated effervescence occurred, and eventually
from 780–800 cubic centim. of gas were collected. The
temperature of distillation slowly rose to 240–243° C., and
the distillate crystallized in the condenser. It was identified
as triethylphosphine oxide, both by its boiling-point, appear-

* Hofmann and Cahours, *Annalen*, vol. civ. p. 15.
† Ibid.

ance, and by the formation of the characteristic compound with iodide of zinc. The gas, when passed through solution of hydrate of barium, gave no turbidity. It was not affected by bromine, and burnt with a luminous flame. The theoretical yield of ethane from the quantity of base operated upon is 822 cubic centim.—a quantity which is sufficiently near to that found to prove that the base decomposes *wholly* in the manner indicated by Hofmann and Cahours.

We have already shown that the hydrate of tetrabenzyl-phosphonium suffers a similar decomposition under the influence of heat, *

$$(C_7H_7)_4POH = (C_7H_7)_3PO + C_7H_8.$$
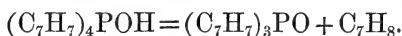
Action of Heat on the Sulphate of the Tetrethylphosphonium.

25 grms. of pure iodide of tetrethylphosphonium were dissolved in water and converted into the corresponding sulphate by the action of sulphate of silver. The solution was filtered and treated with hydrochloric acid and sulphur-retted hydrogen to remove the dissolved silver. It was again filtered and concentrated to a small bulk over the water-bath. During the concentration, a strong smell of triethylphosphine became manifest, showing that decomposition had commenced, and this increased as the concentration was proceeded with ; and even when the solution was removed from the water-bath and placed *in vacuo* over phosphoric anhydride, the smell of triethylphosphine was still apparent.

After remaining under these conditions for some time the solution solidified to a highly deliquescent crystalline mass, which could not be obtained in a fit state for analysis owing to its hygroscopic nature. It was accordingly transferred to a distilling flask and heated in an oil-bath. In a short time gas was evolved whilst a colourless liquid distilled, and there remained behind in the flask a charred mass. The distillate solidified on cooling, and from its appearance we were led to the conclusion that it contained sulphide of triethylphosphine, in addition to the oxide.

A simple method suggested itself for separating the two bodies, depending upon their different solubility in water, the oxide being soluble in all proportions, whilst the sulphide readily crystallizes from a hot saturated solution. The distillate was accordingly dissolved in hot water, and as the solution cooled, beautiful needle-shaped crystals of considerable length separated. They were collected on a filter, washed with cold water, and recrystallized from boiling water.

* Letts and Collie, *loc. cit.*

They possessed the following properties ; they easily volatilized with water vapour, had a melting-point of 94° C., and when heated with sodium yielded triethylphosphine and sulphide of sodium. These properties indicate that the compound was sulphide of triethylphosphine, which was further verified by a determination of sulphur :

0·526 grm. gave 0·840 $BaSO_4$ = 0·1106 S = 21·03 per cent.
Calculated for $(C_2H_5)_3PS$ . . . . . 21·33 per cent.

The mother liquors from which the sulphide of triethylphosphine had separated were evaporated to a small bulk and caustic soda added, when an oily layer separated, containing only a trace of the sulphide. This was removed by redissolving the oily layer in a small quantity of water, and filtering. Caustic soda was then added to the solution, and the oily layer which separated was then decanted and submitted to distillation.

The temperature rose rapidly to 240° C., between which and 244° C. the distillate solidified. The boiling-point, appearance, and other properties left no doubt as to its consisting of oxide of triethylphosphine ; but to be perfectly certain that such was its composition, some of it was converted into the highly characteristic double salt which it forms with zinc iodide, in which a determination of iodine was made.

0·421 grm. required 14·5 cubic centim. of decinormal nitrate of silver solution = 0·18415 grm. iodine = 43·74 per cent.

Calculated for $2\{(C_2H_5)_3PO\}, ZnI_2$ . . . = 43·27 per cent.

The gas collected during the decomposition did not turn lime-water milky, nor was it attacked by bromine. It burned with a luminous flame. The wash-water contained traces of sulphurous acid.

Another experiment was performed in order to see whether any other substances besides the oxide and sulphide of triethylphosphonium could be isolated.

14 grms. of the iodide of tetrethylphosphonium were converted into sulphate, and the latter submitted to the action of heat. 3·5 grms. of sulphide, and 3·0 grms. of oxide of triethylphosphine were obtained ; the theoretical yield [on the assumption that

$\{(C_2H_5)_4P\}_2SO_4$ gives $(C_2H_5)_3PS$, and $(C_2H_5)_3PO$],

is 3·8 grms. of the former and 3·4 grms. of the latter. No other substances could be isolated, so that there can be little doubt that most of the phosphonium salt is decomposed into

oxide and sulphide of triethylphosphine, gaseous hydrocarbons, and oxidized products. As, however, a strong smell of triethylphosphine became manifest during the concentration of the salt, another reaction probably occurs, though to a very small extent. Its exact nature we had no means of ascertaining.

Action of Heat on the Carbonate of Tetrethylphosphonium.

Hofmann and Cahours had already investigated the action of heat on this salt, and state that it decomposes into triethylphosphine and ethyl carbonate.

$$(C_2H_5)_3P\!\!\begin{array}{c}\diagup C_2H_5\\[2pt]\diagdown CO_3\end{array}\ \ =2(C_2H_5)_3P+(C_2H_5)_2CO_3.$$
$$(C_2H_5)_3P\!\!\begin{array}{c}\diagdown\\[2pt]C_2H_5\end{array}$$
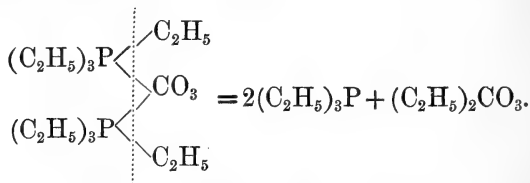
This reaction appeared to be of peculiar interest : first because of its analogy with the behaviour of the compounds of triethylsulphine when heated[*], and, secondly, because it ought to afford an easy and simple method for preparing triethylphosphine from tetrethylphosphonium salts. Unfortunately, however, this decomposition of the carbonate by heat does not occur in the simple manner we anticipated, as the following experiments show.

A considerable quantity of the rather impure carbonate was submitted to distillation. It yielded a large quantity of the *oxide* of triethylphosphine, but only a small amount of the phosphine itself.

In view of this result we determined to investigate the reaction more carefully, and to employ the carbonate in as complete a state of purity as possible. An aqueous solution of 40 grms. of the pure iodide of tetrethylphosphonium was treated with an excess of recently precipitated carbonate of silver. The solution was filtered from the resulting iodide of silver and concentrated on the water-bath, until a slight smell of triethylphosphine became manifest. It was then well shaken with ether to dissolve out any oxide of triethylphosphine that might be present[†], and placed *in vacuo* over phosphoric anhydride, where, after some time, it solidified to a mass of highly deliquescent needle-shaped crystals. This was transferred to an ordinary distilling flask, which was

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[*] Crum Brown and Blaikie, *Journal f. prak. Chemie* [2] xxiii. p. 395.
[†] Ether readily dissolves this body, contrary to the statement of another observer.

connected with a Liebig's condenser, to which an apparatus for collecting gas was attached.   On applying heat to the flask the crystals fused, and soon began to decompose with effervescence.   A few grammes of liquid passed over at a low temperature, but the thermometer (in the distilling flask) soon rose to 240°, between which temperature and 250° the bulk of the contents of the flask distilled.

The distillate consisted of two layers, the lower of which was simply an aqueous solution of oxide of triethylphosphine; the upper layer (which amounted to only a gramme or two) had a strong odour of triethylphosphine.   When shaken with hydrochloric acid about two thirds of it was absorbed ; the remaining third was not attacked, even when boiled with strong hydrochloric acid.   This latter had a distinct and cha-racteristic odour, and its boiling-point lay between 90–100° C. As it was not attacked when boiled with hydrate of barium or caustic soda, and as carbonate of ethyl boils at 126° C., its identity with that substance was clearly negatived.

The gas evolved during the experiment caused a turbidity with lime-water, and was in part absorbable by caustic potash (to the extent of about one third the total volume).   It there-fore contained carbonic anhydride.   The residue was not acted upon by bromine ; and as it burnt with a luminous flame, it probably consisted of a paraffin hydrocarbon, pre-sumably butane.

In order to ascertain the nature of the volatile liquid, which accompanied the triethylphosphine, another experiment was performed with a larger quantity of the carbonate.   On sub-mitting it to the action of heat the same phenomena were observed as before, and a quantity of the volatile liquid (freed from triethylphosphine), amounting to two grammes, was obtained.

After two or three fractionations its boiling-point was found to be between 95°–102° C.   Now this boiling-point agrees fairly well with that of diethylketone (101° C), the formation of which, along with the phosphine oxide, could be accounted for by the equation—

$$(C_2H_5)_3P \overset{\displaystyle C_2H_5}{\underset{\displaystyle O}{\diagdown}} \quad (C_2H_5)_3P \overset{\displaystyle O}{\underset{\displaystyle C_2H_5}{\diagup}} \!\!> CO = 2(C_2H_5)_3PO + (C_2H_5)_2CO.$$
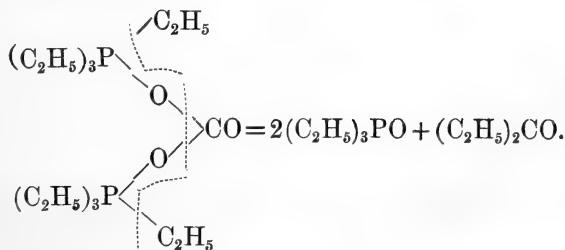
As Schmidt [*] denies that this ketone yields a crystalline compound with bisulphite of soda, we were not surprised to find that the body in question would not unite with the bisulphite. As its quantity was only small we decided to submit it to oxidation. Accordingly it was digested for some time with bichromate of potash and sulphuric acid, and the mixture then submitted to distillation, when an acid distillate was obtained. This was warmed with oxide of silver, filtered and concentrated. On cooling, a white crystalline silver salt separated, in which, when dry, a determination of silver was made.

> 0·161 grm. gave 0·0975 grm. $Ag = 60·56$ per cent.
> Calculated for $C_3H_5O_2Ag$ . . $= 69·66$ per cent.

The mother liquor yielded on concentration another quantity of silver salt, which contained a higher percentage of silver.

> 0·1385 grm. gave 0·085 grm. $Ag. = 61·37$ per cent.
> Calculated for $C_2H_3O_2Ag$ . . $= 64·67$ per cent.

These results indicate that the product of oxidation consisted of a mixture of acetic and propionic acids [†]. The formation of which further proves that the volatile liquid was in reality diethylketone.

We consider, therefore, that part at least of the phosphonium salt decomposes in the manner indicated above.

But another and totally different reaction also occurs, in which triethylphosphine, carbonic anhydride, and a gaseous hydrocarbon are produced. The following equation *probably* represents this second change :—

$$(C_2H_5)_3P\bigg\langle \genfrac{}{}{0pt}{}{C_2H_5}{O}{}\genfrac{}{}{0pt}{}{}{}{CO} \genfrac{}{}{0pt}{}{}{O}{} (C_2H_5)_3P\bigg\langle \genfrac{}{}{0pt}{}{}{C_2H_5}{} = (C_2H_5)_3P + (C_2H_5)_3PO + CO_2 + C_4H_{10}.$$
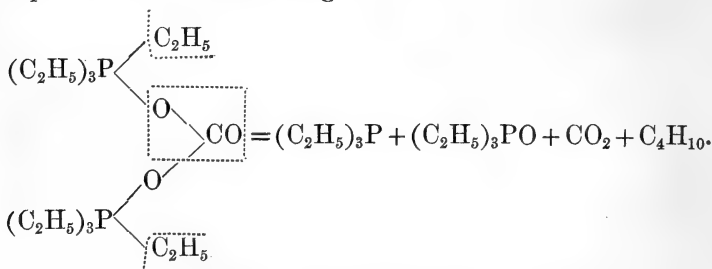
But we could devise no means of proving it absolutely. No trace of carbonate of ethyl could be detected among the products of the decomposition of the phosphonium salt; and we

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[*] Schmidt, *Ber. d. deutsch. Chem. Ges.* v. p. 599.
[†] The analytical numbers are not very satisfactory, but the quantity of material at our disposal was so small that we could not purify the two silver salts.

are thus forced to the conclusion that Hofmann and Cahours were mistaken in their explanation of the change which it suffers when heated.

Action of Heat on the Acid Carbonate of Tetrethylphosphonium.

When a solution of the hydrate of tetrethylphosphonium is saturated with carbonic anhydride, the solution is faintly acid, and solidifies when evaporated over sulphuric acid *in vacuo* to a mass of highly deliquescent needle-shaped crystals. These, on being subjected to the action of heat, decomposed at about 140° C., yielding carbonic anhydride gas, and the salt in the distilling flask turned alkaline in its action on litmus paper ; on further heating, large quantities of gas were evolved between 140° and 150° C. The thermometer then rapidly rose to 240° C., when it was moved up into the neck of the flask; and between this temperature and 250° C. the whole of the contents of the flask distilled.

5 grms. of salt yielded when thus distilled 560 cub. centims. of gases, and a distillate consisting of a little free triethyl-phosphine, some ketone insoluble in hydrochloric acid, and with a boiling-point about 100° C. ; but the chief product was the oxide of triethylphosphine. As in the case of the normal carbonate, we could not detect any carbonate of ethyl. The gases were composed of equal volumes of carbonic anhydride, and a hydrocarbon not absorbable by bromine, which burnt with a luminous flame, and was presumably ethane or butane.

These results show that the decomposition of the acid carbonate occurs in the same manner as that of the normal carbonate, into which it is probably first converted.

Action of Heat on the Acetate of Tetrethylphosphonium.

The interesting and unexpected results obtained with the carbonate induced us to study the action of heat on the acetate of tetrethylphosphonium, as we thought it very probable that it would also yield a ketone, as one half of a ketonic group exists ready formed in the acetyl group.

The acetate was prepared both by acting on the pure iodide of tetrethylphosphonium with acetate of silver, and by neutralizing the hydrate with acetic acid. In both cases the solution of the salt was first evaporated on the water-bath to a small bulk and then placed over sulphuric acid *in vacuo*, and eventually yielded a highly deliquescent crystalline mass.

Some of the salt was dried *in vacuo* over sulphuric acid till constant in weight, and then a combustion was made.

0·203 grm. salt gave 0·428 grm. CO_2, and 0·218 H_2O. C = 57·49 and H = 11·93.

Theory for $P(C_2H_2)_4C_2H_3O_2$.	Found.
C = 58·25	57·49
H = 11·16	11·93.

About 24 grms. of the salt were transferred to a distilling flask connected with a Liebig's condenser and an apparatus for collecting any gases that might be formed.

On applying heat to the crystals they fused, but no apparent change took place until the temperature reached 230° C., when an effervescence occurred and liquid began to distil. Eventually 500 cub. centims. of gas were collected and a large quantity of liquid distillate. This distillate when fractionated yielded two products—one boiling at 80–100° C., the other at 230–250° C. The first had a strong odour of triethylphosphine. It was therefore shaken with hydrochloric acid to remove that body. There remained a volatile insoluble liquid smelling of acetic ether. This was warmed for some time with caustic potash solution until the odour of the ether disappeared. As the liquid suffered scarcely any change in volume when thus treated, the quantity of acetic ether present could only have been very small.

Purified by these processes the liquid now had a slight pleasant odour and a boiling-point which lay between 80–90° C., but as the quantity at our disposal was very small we could not determine it exactly. The boiling-point of ethylmethylketone is 81° C, and yields on oxidation acetic acid only. To establish the composition of the liquid under examination it was submitted to oxidation with bichromate of potash and sulphuric acid, and yielded an acid distillate. The latter was saturated with oxide of silver, and the mixture warmed and filtered; on cooling, a considerable quantity of a silver salt separated out, which had all the appearance of the acetate. The salt was dried and a determination of silver made.

0·914 grm. gave 0·588 grm. Ag = 64·33 per cent.
Calculated for $C_2H_3O_2Ag$ . . . = 64·67 per cent.

The composition of the salt was further verified by warming a portion with sulphuric acid, when the odour of acetic acid became apparent.

The other fraction of the distillate boiling from 230–250° C. yielded on redistillation a considerable quantity of oxide of triethylphosphine, which was identified by its boiling-point,

appearance, and formation of the characteristic double salt with iodide of zinc. There appeared to be present along with the phosphine oxide some substance boiling at a higher temperature than 245° C., but we did not succeed in isolating it in a fit state for examination.

The triethylphosphine was identified in the hydrochloric acid extract of the first fraction (of the original distillate) by the addition of potash in excess, which at once caused the separation of an oily layer. This had the odour of the phosphine, and when warmed in air inflamed. On mixing a portion with iodide of ethyl, heat was developed, and a crystalline compound formed, which analysis showed was iodide of tetrethylphosphonium.

0·501 grm. required 18·2 cub. centims.
   decinormal nitrate of silver solution=
   0·23114 iodine . . . . =46·13 per cent.
Calculated for $(C_2H_5)_4PI$ . . . . . =46·35 per cent.

In another experiment 10 grms. of the acetate gave, when similarly heated, a distillate composed of 6·5 grms. oxide of triethylphosphine, half a gramme of ketone, and 1—1·2 grm. of triethylphosphine, together with 370 cub. centims. of gas, which consisted of nearly equal volumes of carbonic anhydride (90 cub. centims.), an olefine (100 cub. centims.), and a paraffin (110 cub. centims.); the remaining (70 cub. centims.) gas being the air displaced from the condensing apparatus.

In order to ascertain which member of the paraffin series the hydrocarbon was which was not absorbable by bromine, some of the acetate was heated in an apparatus filled with pure carbonic anhydride. The gases evolved during the distillation were therefore free from air; and by subsequent treatment with bromine and caustic potash successively, all the carbonic anhydride and olefine were absorbed, leaving any hydrocarbon of the C_nH_{2n+2} series. The gas which remained after this treatment burnt with a faintly luminous flame, and a rough analysis gave the following results :—

	I.	II.
	cub. cent.	cub. cent.
Gas taken	5·6	11·5
Oxygen and gas	39·0	44·0
Oxygen, gas, and air.	none	11·4
After explosion	33·0	44·5
After addition of caustic potash .	27·0	32·5

Or,—

	Gas taken.	Oxygen consumed.	Carbonic anhydride produced.
	cub. cent.	cub. cent.	cub. cent.
I. . . .	5·6	12·0	6·0
II. . . .	11·5	21·9	11·5

Showing that a volume of the gas requires two volumes of oxygen for combustion, and gives one volume of carbonic anhydride. The gas we therefore concluded to be marsh-gas; for

$$CH_4 + 2O_2 = 2H_2O + CO_2.$$
$$\text{1 vol.} \quad \text{2 vols.} \qquad \qquad \text{1 vol.}$$
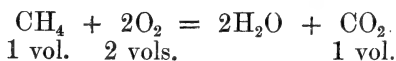
The results just described indicate that, like the carbonate, the acetate of tetrethylphosphonium, when heated, suffers two distinct changes.

The amount of gases yielded by its decomposition (370 cub. centims. from 10 grms. salt) is very small, and is probably formed in the same reaction that produces the triethylphosphine; while the ketone and the phosphine oxide are probably produced by a totally different decomposition.

(1) $(C_2H_5)_3P{<}{\overset{C_2H_5}{\underset{O\,\vdots\,CO\,CH_3}{}}} = (C_2H_5)_3PO + CH_3CO\,.\,C_2H_5.$

(2) $(C_2H_5)_3P{<}{\overset{C_2H_4}{\underset{OCO-}{}}}{\underset{CH_3}{\overset{H}{}}} = (C_2H_5)_3P + CO_2 + C_2H_4 + CH_4.$
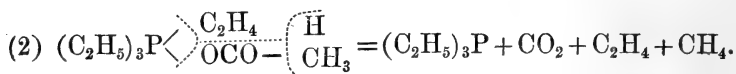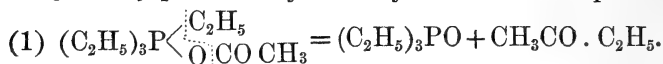
Action of Heat on Benzoate of Tetrethylphosphonium.

The benzoate was prepared by neutralizing hydrate of tetrethylphosphonium with benzoic acid. The solution was evaporated *in vacuo* over sulphuric acid, and eventually gave a solid mass of radiating deliquescent crystals. 10 grms. of the salt were heated. After the crystals had melted at 160° C., white fumes of triethylphosphine appeared in the distilling flask. The thermometer was then raised out of the liquid (when the temperature had risen to 200° C., and decomposition had begun). The temperature of distillation was higher than usual, the thermometer rising ultimately to above 300°, the liquid which condensed in the receiver being of a yellow colour; slight charring also occurred. 450 cub. centim. of gases were collected, consisting of equal volumes of carbonic anhydride and a hydrocarbon of the olefine series, probably ethylene. The liquid distillate was composed of about 5 to 5·5 grms. of oxide of triethylphosphine, about ·5 grm. of free triethylphosphine (soluble in hydrochloric acid), and about 3 grms. of a liquid insoluble in the

acid, and not changed when warmed with caustic soda. As this liquid seemed to consist of at least two substances, more of the benzoate was decomposed, in order to obtain enough of the mixture to be separated satisfactorily by fractional distillation. This did not prove difficult, as one of the constituents boiled below 100° C., while the other possessed a boiling-point slightly above 200° C. Eventually two liquids were obtained, one boiling from 80°–85°, and the other boiling from 210°–215° C. The former had an aromatic sweet smell, and was insoluble in water. A combustion gave the following results :—

0·286 grm. gave ·960 CO_2 and ·213 H_2O.
$$C = 91·54, \quad H = 8·27.$$

	Calculated for C_6H_6.					Found.
C =	92·30	.	.	.	.	91·54
H =	7·69	.	.	.	.	8·27

Thus indicating that the substance was the hydrocarbon benzol.

The liquid possessing the high boiling-point of 210°–215° C. had a sweet pleasant odour, and bearing in mind the results obtained when the acetate was heated, we expected to find phenylethyl ketone, which boils at 210° C., and yields on oxidation acetic and benzoic acids. The liquid was therefore boiled for some time with bichromate of potash and sulphuric acid, and then distilled. The first portions of the distillate were strongly acid, and were neutralized with ammonia. The ammonia salt thus obtained gave with nitrate of silver a crystalline salt, which was recrystallized and analyzed.

0·360 grm. gave 0·225 Ag.. = 62·5 per cent. Ag.
$Ag(C_2H_3O_2)$ . . . . = 64·6 per cent. Ag.

The low percentage of silver found is possibly due to benzoate of silver being present. The chromic-acid solution remaining after the distillation was shaken out with ether, and the etherial solution evaporated. A crystalline acid remained, possessing the characteristic odour of benzoic acid. It was neutralized with ammonia, and precipitated with nitrate of silver solution. The resulting silver salt on analysis gave the following numbers :—

0·345 grm. salt gave ·165 Ag. . = 47·82 per cent. Ag.
$Ag(C_7H_5O_2)$. . . . . = 47·16 per cent. Ag.

As the ketone seemed to be formed in fair amount by this reaction, a considerable quantity of the benzoate was prepared and subjected to the action of heat. In one experiment 10 grms. of salt gave 500 cub. centims. of gas (consisting

of equal volumes of carbon dioxide and ethylene), showing that the reaction producing these two substances was very limited. Eventually several grammes of the supposed ketone were obtained, boiling from 205°–210° C. On combustion the following numbers were obtained :—

I.   0·244 grm. gave 0·6785 grm. CO_2, and 0·166 H_2O.
II.   0·176   ,,   0·4885   ,,   0·1175 H_2O.

	Calculated for		Found.	
	$C_6H_5COC_2H_5$.	$C_6H_5COOC_2H_5$.	I.	II.
C.	80·59	72·00	75·82	75·86
H.	7·46	6·66	7·56	7·41

These results show that the supposed ketone is by no means separated from impurities by simple distillation, and the substance most likely to be formed, and lower the percentage of carbon at the same time, is the ethyl benzoate. As the benzoate could be removed by decomposing it with caustic potash, the remainder of the liquid (B.P. 205°–210° C.) was boiled with a solution of hydrate of potassium, washed with water, dried, and redistilled. The boiling-point still remained the same (205°–210° C.), but a combustion gave a much larger amount of carbon—

0·240 grm. gave 0·702 grm. CO_2 and 0·1603 H_2O.

	Calculated for $C_6H_5COC_2H_5$.	Found.
C	80·59	79·76
H	7·46	7·42

Another quantity of the benzoate was prepared by the action of benzoate of silver on the iodide of tetrethylphosphonium. On heating the salt, the same phenomena were observed as with the benzoate prepared by the action of benzoic acid on the hydrate. On fractionating, however, the portion of the liquid distillate which was soluble in water, a quantity boiling from 120°–140° C. was obtained. On removing the triethylphosphine by shaking with dilute hydrochloric acid, an aromatic-smelling liquid remained, which had an odour somewhat like that of cymol, and a boiling-point 130°–140° C.

Whether this liquid was ethyl benzol or not we were unable to determine, as the quantity at our disposal was too small. The next fraction collected boiled at 200°–215° C., and was insoluble in water ; and on oxidation with chromic acid yielded acetic and benzoic acids. The last portions of the distillate were pure oxide of triethylphosphine.

As this result was slightly different from those at first ob-

tained, still another experiment with considerable quantities (40–50 grms.) of the benzoate was made. It was prepared by treating a solution of the hydrate with benzoic acid. The solution was evaporated as far as possible on a water-bath, transferred to a distilling flask, and heated. Slight decomposition occurred before 200° C., but it did not become brisk until the thermometer rose above 300°. The products of the reaction consisted of an aqueous solution of oxide of triethylphosphine, above which floated a yellow oil. On distilling the former, eight grammes of triethylphosphine oxide were obtained ; the oily liquid weighed about 32 grammes. It was shaken with hydrochloric acid to remove triethylphosphine (which, when separated by potash, weighed nearly 8 grammes). The oil, after this treatment, was distilled, and yielded 4–5grammes of benzol boiling from 78°–90° C., and sufficiently pure to solidify in a freezing-mixture. The higher boiling portions consisted chiefly of a liquid smelling strongly of benzoic ether, and boiling from 208°–220° C. On treating this with ammonia gas it almost solidified into a crystalline mass of benzamide. This was removed by boiling water, and the oily liquid which remained once more fractionated, when it all passed over between 208°–214° C. A combustion gave the following results :—

0·3347 grm. gave 0·952 grm. CO_2 and 0·2235 grm. H_2O.

Carbon = 77·5 per cent. Hydrogen = 7·4 per cent.

The hydrocarbon ethyl benzol was carefully looked for in this experiment, but could not be detected.

The production of triethylphosphine, its oxide, ethylene, benzol, ethylphenyl ketone, and carbonic anhydride, indicate that the benzoate decomposes when heated in a similar manner to the acetate and suffers two distinct changes—the one yielding the free phosphine, the other yielding the oxide. The two reactions may be represented by the following equations [*] :—

(1) $(C_2H_5)_3P{<}^{C_2H_5}_{O\,COC_6H_5} = (C_2H_5)_3PO + C_6H_5COC_2H_5.$

(2) $(C_2H_5)_3P{<}^{C_2H_4\,H}_{OCO\,C_6H_5} = (C_2H_5)_3P + CO_2 + C_2H_4 + C_6H_6$;

and

$(C_2H_5)_3P{<}^{C_2H_5}_{OCOC_6H_5} = (C_2H_5)_3P + C_2H_5OCOC_6H_5.$

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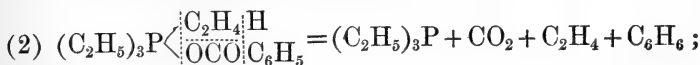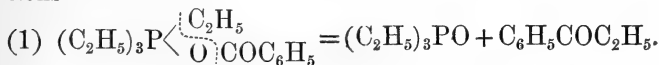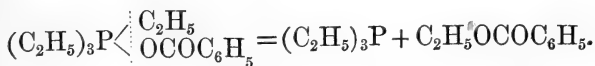
[*] The production of ethyl benzol could be explained by the equation :—

$(C_2H_5)_3P{<}^{C_2H_5}_{OCO\,C_6H_5} = (C_2H_5)_3P + CO_2 + C_6H_5C_2H_5.$
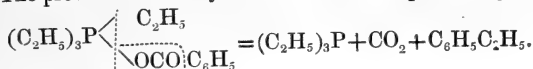
Action of Heat on the Oxalate of Tetrethylphosphonium.

This salt was prepared by the action of oxalate of silver on a solution of the iodide of tetrethylphosphonium. The filtered solution was evaporated over a water-bath, and the residue dried *in vacuo* over sulphuric acid. It solidified to a mass of fine crystals. Some of these were allowed to remain *in vacuo* till of constant weight, and then a combustion was made.

> (I.) 0·270 grm. salt gave 0·553 grm. CO_2, and
> 0·253 grm. H_2O.   C = 55·85.   H = 10·41.

Another quantity of the solution of the oxalate was evaporated in a distilling flask, and finally the temperature of the solution was raised to 160° C., when no decomposition occurred ; the liquid on cooling solidified to a mass of crystals. Some of these were analyzed.

> (II.) 0·315 grm. salt gave 0·285 grm. H_2O, and
> 0·658 grm. CO_2.   C = 56·96.   H = 10·05.

	Calculated for $[P(C_2H_5)_4]_2C_2O_4$.	Found.	
		I.	II.
C	56·56	55·85	56·96
H	10·47	10·41	10·05

This salt, therefore, appears to be stable, and can be heated to 160° C. without any change. Decomposition, however, begins slowly at about 200° C., and at 230° C. gas is rapidly evolved.

6·5 grms. salt when subjected to the action of heat gave 650 cub. centims. of gases ; and the whole of the contents of the flask distilled without charring. The distillate was chiefly oxide of triethylphosphine, together with some triethylphosphine, and a liquid insoluble in water and hydrochloric acid, which possessed a boiling-point 95°–105° C., and which gave on oxidation acetic acid. It resembled in smell and properties the diethyl ketone, obtained when the carbonate was heated, but it was formed only in small quantity.

The 650 cub. centims. of gases were first treated with bromine, when 100 cub. centims. were absorbed; caustic potash further reduced these by 100 cub. centims., leaving 450 cub. centim. The remaining gases burnt with a bluish and scarcely luminous flame.

Several analyses were made of the gas evolved by the decomposition of the oxalate, two of which are subjoined.

	I.	II.
	cub. centims.	cub. centims.
Gas taken	8·9	10·0
Oxygen and gas	36·0	39·1
After explosion	31·6	34·2
After addition of caustic potash .	23·0	24·5

Which give :—

	Gas taken. cub. centims.	Oxygen consumed. cub. centims.	Carbonic Anhy- dride produced. cub. centims.
I. . .	8·9	4·1	8·6
II. . .	10·0	4·6	9·7

Allowing for the roughness of the experiment and the probable slight impurity of the gas experimented upon, the above numbers agree fairly well with the ratio in volumes.

Gas taken.		Oxygen consumed.		Carbonic Anhydride produced.
2	:	1	:	2

proving that the gas consisted of carbonic oxide ; for

$$2CO + O_2 = 2CO_2.$$
$$2 \text{ vols.} + 1 \text{ vol.} = 2 \text{ vols.}$$
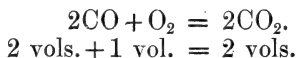
From these results we consider it probable that the decomposition of the oxalate can be expressed by the following equations :—

(1)

$$\begin{array}{c} (C_2H_5)_3P \diagup O \cdots CO \\ \cdots \\ (C_2H_5)_3P \diagdown O - CO \diagup C_2H_5 \end{array} = 2(C_2H_5)_3PO + (C_2H_5)_2CO + CO.$$

(2)

$$\begin{array}{c} (C_2H_5)_3P \diagup O - CO \diagup C_2H_4 H \\ \cdots \\ (C_2H_5)_3P \diagdown O - CO \diagup C_2H_4 H \end{array} = (C_2H_5)_3PO + (C_2H_5)_3P + 2C_2H_4 \\ + 2CO + H_2O.$$

(3) $[(C_2H_5)_4P]_2C_2O_4 = (C_2H_5)_3PO + (C_2H_5)_3P + (C_2H_5)_2CO + CO_2.$

(4) $[(C_2H_5)_4P]_2C_2O_4 = 2(C_2H_5)_3P + 2C_2H_4 + H_2O + CO_2 + CO.$
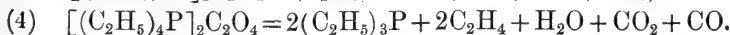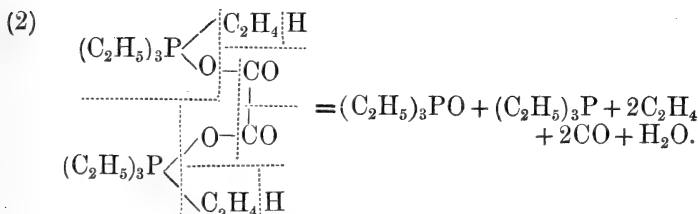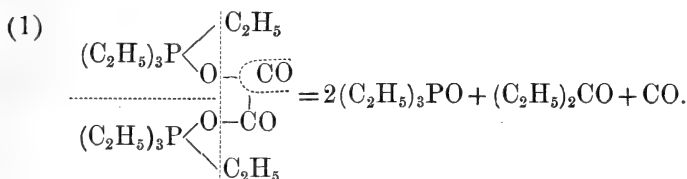
Several attempts were made to prove the existence of ethane in the carbonic oxide gas, and numerous analyses of

the gas were made without success ; thus proving that any of the ethyl groups which are split off from the phosphorus atom, if they do not form diethyl ketone, lose a hydrogen atom and are evolved as ethylene gas.

An interesting reaction was noticed between free sulphur and the oxalate. The salt, when fused and heated gently to about 160° C., gives, with free sulphur, a splendid deep indigo-blue colour, which turns green and finally yellow on cooling. The colour, however, reappears on heating ; but if too strongly heated the sulphide of triethylphosphine is formed, and total decomposition of the oxalate occurs. Many substances containing sulphur, such as vulcanized indiarubber, gunpowder, &c. give this blue colour when even the smallest particles are heated with the oxalate on a porcelain crucible lid. Most of the other salts of tetrethylphosphonium give this colour with free sulphur when warmed, but none gave it in such a marked manner as the oxalate.

It is remarkable that the oxy-salts of tetrethylphosphonium should so easily give sulphide of triethylphosphine when fused with sulphur, while the oxide of triethylphosphine itself is totally unacted upon by sulphur under similar conditions.

Action of Heat on the Cyanide of Tetrethylphosphonium.

As all the preceding experiments were performed with salts of the phosphonium derived from oxy-acids, and as in each case part of the oxygen was eliminated in the form of oxide of the tertiary phosphine, we considered that it would be of interest to study the action of heat on some of the salts of the phosphonium which contained no oxygen, and which therefore could not yield that body.

We fixed upon the cyanide to commence with as being likely to decompose at a lower temperature than any other haloid compound.

The cyanide of tetrethylphosphonium was obtained in solution by double decomposition between the iodide and cyanide of silver. It was found that when this solution was concentrated on a water-bath partial decomposition occurred, and hydrocyanic acid was evolved together with an inflammable gaseous hydrocarbon. This decomposition being no doubt due to the action of the water on the cyanide, probably in the following manner :—

$$(C_2H_5)_3P{<}^{C_2N_5}_{CN} + H_2O = (C_2H_5)_3PO + HCN + C_2H_6.$$
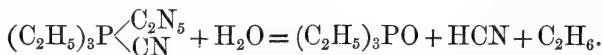
The solution was transferred to a distilling flask and heated, when hydrocyanic acid, cyanogen gas, a little free triethyl-

phosphine, and some oxide of triethylphosphine were pro-
duced. Probably also a gaseous hydrocarbon was evolved.
Mixed with the triethylphosphine and insoluble either in
water or hydrochloric acid were a few drops of a liquid smell-
ing of acetonitrile, and evolving ammonia when boiled with
caustic potash. The production of free phosphine and aceto-
nitrile indicate that part of the cyanide at least decomposes
according to the equation

$$(C_2H_5)_3P{<}^{C_2H_5}_{CN} = (C_2H_5)_3 + C_2H_5CN.$$
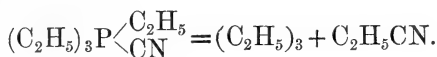
The decomposition, however, is complex, owing to the high
temperature at which it occurs.

We may mention that the cyanide when heated with cold
caustic-potash solution is not changed, and appears to be
quite insoluble if the solution is strong. On warming the
mixture a copious evolution of inflammable gas occurs, and
the whole of the cyanide decomposes according to the
equation.

$$(C_2H_5)_3P{<}^{C_7H_5}_{CN} + KOH = (C_2H_5)_3PO + KCN + C_2H_6.$$
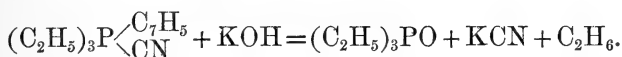
The action of heat on the iodide was also investigated: it
first fuses, but does not suffer any further change until the
temperature has risen above the boiling-point of mercury.
It then turns brown and splits up into hydriodic acid, free
phosphorus, and gaseous products, and a charred residue
remains.

The decomposition is therefore complex, and cannot be ex-
pressed by an equation.

Although the iodide is not attacked by an aqueous solution
of caustic potash, yet when boiled with a concentrated alco-
holic solution it slowly decomposes in the same manner as
the cyanide.

Action of Heat on the Chloride of Tetrethylphosphonium.

After finding that neither the cyanide nor the iodide of
the phosphonium when heated yield triethylphosphine in large
quantities, it was hardly thought probable that by the action
of heat on the chloride the tertiary base would be produced.
The results obtained were, however, of the most unexpected
nature. 13·5 grms. of the phosphonium iodide were con-
verted first into hydrate, and then, by careful addition of
hydrochloric acid till the solution was neutral, into the chlo-
ride. The solution was evaporated on the water-bath until

it became concentrated, and was then transferred to a desic-
cator and allowed to remain there till it became a mass of
solid very deliquescent crystals.

A chlorine determination was made with some of these crys-
tals, which were dried *in vacuo* till constant in weight.

I.   0·198 grm. took 11·0 cub. centims. of decinormal nitrate
                of silver solution = 19·7 per cent. Cl.

II.   0·2935 grm. took 16·1 cub. centims. of decinormal nitrate
                of silver solution = 19·4 per cent. Cl.

	Found.	
Calculated for		
$(C_2H_5)_4PCl$.	I.	II.
per cent.	per cent.	per cent.
Cl = 19·4	19·7	19·4.

7 grms. of this solid chloride were heated in a small flask
connected with the ordinary apparatus used for collecting
any gases evolved.   The thermometer in the liquid rose to
300° C. before any decomposition of the salt began. . It was
then raised into the neck of the flask.   As the decomposition
continued a white powdery crystalline substance condensed
in the neck of the flask, and great difficulty was found in
preventing the exit-tube from becoming completely choked
by this salt ; for on attempting to melt it with a gas-flame it
only sublimed onto another portion of the tube.   Eventually
the whole of the chloride was distilled out of the flask, and
yielded, besides this solid distillate, 800 cub. centim. of gases.
This gas was completely absorbed by bromine, yielding a
liquid smelling like ethylene bromide and boiling between
130°–140° C.

Some of the white crystals were quickly scraped out of the
condenser and weighed and analysed.

0·310 grm. took 21 cub. centims. of decinormal nitrate
                of silver solution = 22·99 per cent. Cl.

The solution of the crystals in water was just acid to lit-
mus paper, and when treated with caustic-soda solution
gave free triethylphosphine.   From the above results and
from the above analysis there was, therefore, no doubt that the
substance was the *hydrochlorate of triethylphosphine* which
contains 22·98 per cent. of chlorine, and which, when treated
with caustic soda, yields the free base.   The decomposition
therefore is expressed by the equation,

$$(C_2H_5)_3P{\scriptstyle\diagup}^{H\,\overline{C_2H_4}}_{Cl} = (C_2H_5)_3PHCl + C_2H_4,$$
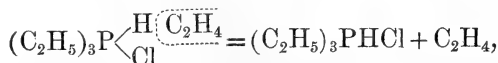
the amount of gas produced in the decomposition, namely

800 cub. centim., agreeing with the amount demanded by theory, 860 cub. centim.

The remainder of the distillate was dissolved in water, and had a faintly acid reaction; on addition of caustic soda an oil separated, which by its properties and boiling-point, 125°–133° C., was triethylphosphine. The amount of free tertiary base obtained was about three grammes. The hydrochlorate of triethylphosphine obtained by this reaction is a white cystalline powdery solid, which scarcely melts when heated to about 210° C., at which temperature it sublimes or probably dissociates, for if any air be in the tube dense white clouds are formed of the oxide of triethylphosphine. As this reaction is the only one which will yield considerable quantities of the tertiary base from the phosphonium salt, another experiment was made, with every precaution to see whether the yield was quantitative.

A solution of chloride was quickly evaporated, and finally heated in a weighed distilling-flask till the temperature of the liquid had risen to 180° C.; suddenly the whole frothed up and solidified in the flask to a mass of white crystals. The amount of salt thus obtained was 8 grms. This solid mass of crystals did not melt till the temperature was about 270° C., and no decomposition was noticed till the melted salt was at a temperature of 340° C., and gas was not rapidly evolved till the liquid was above 360° C.; 910 cub. centims. of gases were evolved, and the same white distillate was obtained. There was no charring, and the whole of the salt was distilled out of the flask.

The distillate was washed out of the condenser with a little water and transferred to a flask, through which a current of hydrogen was passed; excess of caustic soda was added, and the triethylphosphine which separated was distilled in a current of steam ; 4·3 grms. were obtained, the boiling-point of which lay between 120°–135° C., and when added to ethyl iodide gave the iodide of tetrethylphosphonium.

Part of the gases were treated with bromine, when they were completely absorbed, giving dibromide of ethylene, B.P. 130°–135° C. Another portion was analysed :—

	cub. centim.
Gas taken	4·2
Gas + oxygen	25·6
After explosion	17·0
After addition of caustic potash .	8·7

Or,

Gas taken.	Oxygen consumed.	Carbonic Anhydride produced.
4·2 c.c.	12·7 c.c.	8·3

Approximately the ratio

$$1 \qquad : \qquad 3 \qquad : \qquad 2 \, ;$$

and indicating that the gas was ethylene ; for

$$C_2H_4 + 3O_2 = 2CO_2 + 2H_2O \, .$$
$$1 \text{ vol.} + 3 \text{ vols.} = 2 \text{ vols.}$$

From these results it will be seen that the chloride of tetrethylphosphonium, when heated, decomposes *nearly quantitatively into the hydrochlorate of triethylphosphine and ethylene gas.*

8 grms. of the salt should yield theoretically 980 cub. centim. of ethylene gas and 5 grms. of triethylphosphine; the amounts obtained were 910 cub. centim. of pure ethylene and 4·3 grms. of the free phosphine. This reaction we consider to be of great interest, as affording a ready method for obtaining a tertiary phosphine from a phosphonium derivative.

Some of the other salts of tetrethylphosphonium with inorganic acids were prepared, and the action of heat on them was also investigated.

The *sulphide* and *hydrosulphide of tetrethylphosphonium* were obtained by the action of sulphuretted hydrogen on a solution of the base in water. The solutions were strongly alkaline, and when evaporated *in vacuo* over sulphuric acid gave highly deliquescent needles. Their solution in water precipitated metals like potassium sulphide, and the sulphides of arsenic and antimony were soluble in excess of the solution. Sulphur also dissolved easily, with a yellow colour, forming most probably a persulphide. When a concentrated solution of the sulphide was warmed with ethyl iodide, double decomposition occurred, sulphide of ethyl being produced and iodide of the phosphonium.

When the aqueous solution was concentrated in a distilling flask, as soon as all the water had distilled, the sulphide for a moment at 150°–160° C. turned a beautiful deep indigo-blue colour. This disappeared on further heating ; at 220° C. gas being evolved, together with triethylphosphine and sulphide of triethylphosphine. Charring also occured.

The hyposulphite of tetrethylphosphonium crystallizes well from a concentrated solution in plates, and when heated the same deep indigo-blue colour was noticed as with the sulphide. The decomposition is complex, as much charring occurs : sulphide and oxide of triethylphosphine are among the products of the distillation.

The *sulphocyanate of tetrethylphosphonium* crystallizes in highly deliquescent needles, and when heated does not decom-

pose to any extent till the temperature has risen above 300° C.; it then slowly chars, yielding gases and sulphide of triethylphosphine.

The *nitrate of tetrethylphosphonium*, when evaporated *in vacuo* over sulphuric acid, is deposited from its solution in deliquescent needle-shaped crystals. These, when heated, suddenly puff, yielding large quantities of gases and some oxide of triethylphosphine.

Conclusion.

From the foregoing experiments it appears that the salts of tetrethylphosphonium with organic oxyacids suffer, as a rule, at least two, and occasionally three, different and distinct changes under the influence of heat.

In one of these the molecule splits up into three new groups, consisting respectively of carbonic anhydride, a paraffin hydrocarbon, and a tertiary phosphine. In another, two hydrocarbons are formed, namely, an olefine and a paraffin, in addition to carbonic anhydride and the tertiary phosphine. Whilst in the third, a totally different change occurs, in which only two products are formed, namely, the oxide of the tertiary phosphine and a ketone.

$$(1) \quad Et_3P\big\langle{}^{C_2H_5}_{OOC}R = Et_3P + CO_2 + C_2H_5R.$$

$$(2) \quad Et_3P\big\langle{}^{C_2H_4\,|\,H}_{OOC|R} = Et_3P + CO_2 + C_2H_4 + RH.$$

$$(3) \quad Et_3P\big\langle{}^{C_2H_5}_{O\,|OCR} = Et_3PO + C_2H_5 . CO . R.$$
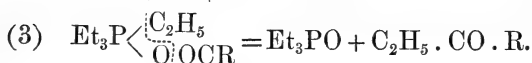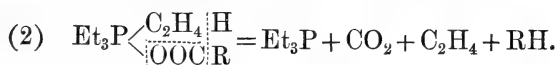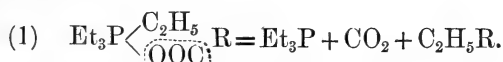
It is possible, if not indeed probable, that the third reaction occurs subsequently to the first, and that it really depends upon the reducing action of the triethylphosphine upon the carbonic anhydride, at the high temperature at which the decomposition usually occurs, whereby carbonic oxide is liberated, which combines with the hydrocarbon radicals in *statu nascendi*, forming a ketone :—

$$Et_3 + P + CO_2 + (Et) + (R) = Et_3PO + Et\, CO\, R$$
$$(\text{or } Et_3P + (OCO - R) + (Et) = Et_3PO + Et\, CO\, R).$$
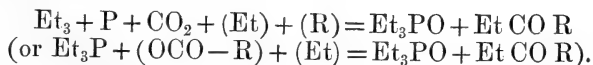
If we merely consider the third kind of decomposition alone, it appears to be, to a certain extent, analogous to the decomposition which a sulphine compound suffers when heated, the difference depending on the far more powerful attraction which phosphorus has for oxygen, than sulphur has for the same element. In both cases a hydrocarbon group is

detached from the molecule, and also the residue of the acid, but whilst with the sulphur compounds these two simply combine (forming a compound ether), and leave a hydrocarbon sulphide, in the case of the phosphonium salt the acid residue is reduced by the tertiary phosphine, and the group thus left combines with the hydrocarbon radical, forming a ketone.

Thus :—

$$Et_2S{<}{Et \atop OOC~R} = Et_2S + Et~OOC~R.$$

$$Et_3P{<}{Et \atop O~OC~R} = Et_3PO + Et~OC~R.$$
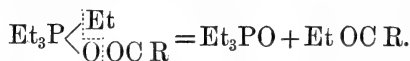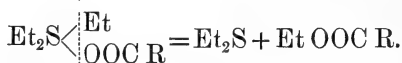
A result of this kind is in perfect harmony with the views already expressed by one of the authors* regarding the analogies and differences existing between phosphorus and sulphur and their compounds.

As regards the action of heat on the hydracid salts of tetrethylphosphonium, the chloride and cyanide alone afforded interesting results. In the case of the cyanide these, however, were complicated by the presence of water, which attacked the compound and converted it into hydrocyanic acid, phosphine oxide, and ethane. A small quantity of it, however, appeared to behave like a sulphur compound, as it was apparently converted into cyanide of ethyl and triethylphosphine. The action of heat on the chloride seems to be anomalous, and as it is the only method by which the tertiary phosphine can be obtained in any quantity from a phosphonium salt, the decomposition of phosphonium chlorides containing various hydrocarbon radicals has been the subject of a separate research, the results of which one of the authors hopes soon to be able to publish.

XXIII. *On Dew.* By John Aitken.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE read the "Remarks on a new Theory of Dew," by Mr. Charles Tomlinson, F.R.S., in your Journal for June ; I have also read the paper there referred to, and written by the same author in the 'Edinburgh New Philosophical Journal,' "On the Claim of Dr. Wells to be regarded as the Author of the 'Theory of Dew.'" In certain respects and up to a certain point, these two papers bear a strong resemblance to

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* E. A. Letts, Trans. Roy. Soc. Edin. vol. xxx. part 1, p. 285.

each other. The aim and object of the greater part of the first of these two papers appears to be to detract from the merit of Dr. Wells's great work, by endeavouring to show that almost all the facts observed by him had been previously noted by other investigators. As Mr. Tomlinson, however, just at the end of his paper, reinstates Dr. Wells in the position he previously occupied, perhaps nothing further need be said on that part of the subject; but as he has not been equally generous to me, I must undertake the uncongenial task of trying to put myself right with your readers. I will take Mr. Tomlinson's remarks in the order of his paper, and as your space is valuable, will make my reply as short as possible.

At the very outset Mr. Tomlinson, by raising a false contention, attempts to place me in opposition to recognized authorities, by entitling his paper " Remarks on a new Theory of Dew." The results of my investigation are in no sense entitled to be called, and never have been called by me, a " *New Theory*;" nor was this so-called " new Theory of Dew" " promulgated in opposition to that of Dr. Wells." My work has not resulted in a new theory; it is only an extension of the work the foundations of which were laid by Dr. Wells. The great fundamental principles established by that authority are unaffected by anything contained in my paper.

When Dr. Wells wrote his celebrated " Essay on Dew," he must have been quite aware of the uncertainty as to the source of the vapour. Although he appears to have thought that but little of it could rise from the ground while dew was forming, yet with admirable caution he adds, " He was not acquainted with any means of determining the proportion of this part to the whole," thus clearly recognizing the soil as a possible source of the vapour. Wells's theory, however, has principally to do with the condensation of the vapour after it is in the air, and but little with its source.

While quite agreeing with the author as to the difference between " literary work " and " scientific work," and while he in his comparison would extol the literary, I would rather be inclined to consider the difference between literary and scientific work to be so great that they cannot be compared. Both are useful in their way. Mr. Tomlinson no doubt has had sufficient experience as a thinker, a writer, and an investigator to have seen many a fair theory, apparently perfect in all its parts, and consistently thinkable all through, vanish, under the light of experimental test, like the baseless fabric of a dream at the first touch of day.

At pages 485–6 Mr. Tomlinson clearly states the chief points, established by Dr. Wells, under six heads, and then

with equal clearness gives under eight heads what he calls Mr. Aitken's theory of dew. If he had entitled these last as eight points contended for in my contribution to the theory of dew, no fault could be found with this part of his paper, as he states the points with clearness and precision. But unfortunately he still looks on these eight points as rivals to the previous six of Dr. Wells, and so continues hostile to them. "Such," he says, "is the new theory of dew, which, if accepted, must go far to render nugatory the results obtained by some of the most celebrated observers." After reading Mr. Tomlinson's paper, however, I do not find that he adduces any results of previous observers that are in any way rendered nugatory by the results set forth in my paper. It has already been stated that the "new theory" is not in opposition nor are the results contrary to the teaching of Dr. Wells; nor are they, so far as I know, contrary to those of any of the "celebrated observers."

In continuation, my critic then, in place of sustaining his accusation by giving examples in which my teaching is at variance with recognized authorities, gives an example in which my observations agree with recognized authority. At the foot of p. 486 he objects to the statement in my paper that "the ground at a short distance below the surface is always hotter than the air over it," because it is not a new observation, Pictet in 1779 having previously observed the fact. I am, however, puzzled to understand what bearing this has on the subject, or what conclusion he wishes to be deduced from this statement. To compare small things with great : more than one person had seen an apple fall to the ground before Newton made such good use of the observation. Pictet, when he made this observation, was working at another subject, and did not draw any conclusion which would be rendered nugatory by the acceptance of my conclusions.

Mr. Tomlinson proceeds to say, "that dew rose out of the ground is a very old notion." I was well aware that the idea was an old one, and it is distinctly stated in my paper. It is difficult to understand what impression my critic wishes to convey by this remark. If it is that the idea does not possess the merit of novelty, having been already expressed, my reply is that it has been already said that dew was caused by the moon-beams, that it descended from the stars, &c. ; and each and all of these ideas were supported by about the same amount of evidence, namely the *ipse dixit* of the theorist.

The same answer applies to the footnote, p. 486, regarding the exudation of moisture by plants. Though "Muschenbroeck regarded dew as a real perspiration of plants," yet

beyond the bare statement I am not aware of anything he advanced to support his idea. His theory thus fails to explain the difference between the dew-drop on plants and true dew. Nor does it account for dew on dead matter. On these historical matters I am, however, open to conviction, as it is extremely difficult to get access to all that has been written on this very popular subject.

As I nowhere say that vapour may not be condensed out of air under conditions other than those where dew is forming, the two paragraphs at the foot of page 487 have no bearing on the subject.

I am unable to see the relation between the observations made by Wells and Six and myself, which Mr. Tomlinson brings together by the footnote at page 488. In the investigations made by the two former observers, the difference remarked was between the readings of thermometers placed on the grass and other substances, and thermometers hung above them ; while in my experiments some thermometers were placed *on* and others *under* the grass. The only likeness in the two sets of observations is a similarity in the amount of the difference in the readings of the respective sets of thermometers.

It is unnecessary to follow Mr. Tomlinson through the natural processes as described in pages 489, 490, further than to say that the cooling he there describes as taking place on grass at night is not correct. At the middle of page 490 he says :—" We may imagine three layers of air—one in contact with the points of the grass ; the second immediately below it, where the blades are more numerous, and more or less exposed to the zenith ; and the third entangled in the matted portion, which is entirely sheltered from the sky." He then describes the manner in which he supposes these layers to get cooled, one after the other, beginning at the upper, till " in the end the lower stratum will be colder than the first, so that the blades and stems which are least exposed to the aspect of the sky will be colder than the points of the blades, and the thermometer buried in the grass will mark a lower temperature than one in contact with the surface." If Mr. Tomlinson had read my paper carefully, or practically put the question to nature, he would not have ventured such an opinion, as he would not, I expect, ever have found the "thermometer buried in the grass mark a lower temperature than the one in contact with the surface." On the contrary, all through the night the lower thermometer remains warmer than the upper one. Mr. Tomlinson, in describing the progress of cooling, appears to have forgot to take into account the supply of heat given

off by the earth. This supply is quite sufficient under all
conditions, so far as I have observed, to maintain the soil and
the thermometer over it at a temperature much above that at
the top of the grass ; and while the thermometer at the top
of the grass falls quickly at night and falls a good deal, the
one " buried in the grass " falls comparatively little, the upper
one remaining much the colder throughout the night. It is
not till day arrives and the temperature rises that the upper
thermometer gets heated and rises to the temperature of the
lower one.

With regard to the remarks about dew in " Persia " and
" the African desert," I have nothing to say, as they do not
bear on my paper, it being distinctly stated there that my
remarks apply only to this climate. I may, however, add
here that I wait for further information before forming any
opinion as to what takes place in other and unknown con-
ditions.

At page 493, Mr. Tomlinson says, " There is such a vast
consensus of scientific opinion in favour of the received theory
of dew, that any attempt to set it aside in favour of another
must be supported by the strongest experimental evidence."
Now, as I have already said, I have never made any attempt
to set aside Wells's theory; and as all my work has tended to
confirm and extend his, I hope Mr. Tomlinson will, on recon-
sideration, give my contribution to this subject a more favour-
able reception ; and might I also ask a more careful consider-
ation of that part referred to by him in this same paragraph,
where he says, " Mr. Aitken exposes a turf six inches square
to the air in a scale-pan " ! ! This misconception of the essen-
tial conditions of the experiment has given rise in his mind
to the objection to the conclusion I have drawn from the
experiment, and has also given rise to the confusion Mr. Tom-
linson has made in comparing my experiment with other
previous ones.

If Mr. Tomlinson had delayed till he had seen the complete
paper, he probably would not have found the difficulty he has
experienced in reconciling the two sets of observations referred
to in the first paragraph, page 494, of his paper. The paper
containing the account of my experiments, on which Mr.
Tomlinson founds his criticism, being in abstract was made as
short as possible, and does not contain sufficient information
to make the conditions of the experiments clear to Mr.
Tomlinson. The edges of the trays were, as he supposes, in
contact with the ground; one slate and one weight were
also in contact with the ground. But, in referring to the
latter, they were simply described as they appeared without

touching them, that is without lifting them from the ground; whereas it is the condition of the undersides or insides of the trays that are specially referred to. The slate and weight resting on the ground were of course quite wet on their undersides, which were invisible except when moved, while their exposed parts were dry ; whereas the elevated slate and weight were wet all over. With this explanation I have no doubt Mr. Tomlinson will see the importance of the contact with the ground in keeping the temperature of the slate and weight above the dew-point; while the wetness of their undersides, which are invisible, brings these experiments into harmony with the experiments with the trays.

In the second last paragraph of Mr. Tomlinson's paper he seems to object to that part of the " new theory " which relates to the excretion of liquid by plants, principally because it is " startling " and not in keeping with preconceived opinions, and he asks, " for what purpose are plants endowed with such high radiating powers?" I scarcely think he can expect me to answer that question, as it forms no part of my theory. If any one, surely he is the proper person to give the reply. But are plants really such good radiators? Take even the figures given by Mr. Tomlinson for the different substances named. In none of these is the difference of any importance; indeed the differences are not more than might easily arise from errors of observation by the method used by Melloni, except in the case of vegetable mould. So far as my own measurements go, by the method used by me, I have not found any difference in the radiating powers of grass and soil at night. If there is any difference, it must be very small. One result of recent investigations is to show that plants with leaves like those of grass are very badly adapted for collecting dew, the blades being little narrow strips placed at right angles to the direction of the air-currents ; the slightest movement of the air prevents the surfaces of the blades being cooled by radiation much below the temperature of the air. In illustration of this I may mention that *narrow strips* of glass or other material exposed at night will sometimes collect no dew, while a *sheet* of the same material will be running with water, the windward edges being of course dry. Plants with large leaves have quite an advantage over grass in dew-collecting powers ; but, on the other hand, grass has the advantage during sunny days, on account of its narrow blades being but little heated by the sun. This may be one reason, though not the only one, why grass so seldom flags under a scorching heat.

If I might be allowed to make a slight explanation here,

perhaps it would help to remove a difficulty some have felt in accepting some of the conclusions contained in my paper. One of the results contended for is, that dew on grass and on bodies near the ground is formed from vapour rising from the ground during night ; the reason given for this conclusion being that grass-land is always in a condition to give off vapour during dewy nights. The vapour that rises from the ground after sunset will thus displace the vapour that rose during the day, and the latter will diffuse itself into the drier air over the grass. The stems and blades of the grass during night will thus be surrounded by the vapour that has risen most recently from the ground. Another reason given is, that at night the temperature of the air among the stems is much higher than that of the air at the tips of the blades, and being in contact with moist soil is nearly saturated. The vapour-tension of this hot air, rising among the stems, is thus much higher than that of the air over them, and is thus in a much more favourable condition for forming dew than the air higher up.

These are the reasons given for concluding that dew on grass is formed from vapour rising at the time from the ground and not from that which rose during the day. The principal difficulty experienced by some in accepting this conclusion, seems to have arisen from extending this conclusion to the source of the dew deposited on bodies placed some distance from the ground. Now, when we consider what takes place in these higher positions, it is easy to see that the conditions are much more complicated, and we can now say very little about the vapour, either as to the place of its evaporation or as to the time when it changed its state. Whenever the vapour coming from the ground rises above the grass and mixes with the drier air over it, we get an entirely different and much more complicated condition of matters. No doubt some of the vapour molecules in this upper air will have risen from the ground only a short time before, but some of them will certainly, if there is the slightest wind, be molecules which have risen during the day, and no doubt some of them will have risen into the air many days previously ; and while some will have risen from the ground immediately underneath, others will have come from lands and oceans far away. But while this may be so, it in no way affects the conclusion that vapour is almost constantly, night and day, given off by the soil, and that dew on grass is part of this rising vapour trapped by the cold blades.

Darroch, Falkirk,   Yours truly,
    June 14, 1886.   John Aitken.

XXIV. *On the Electromotive Force of Voltaic Cells having an Aluminium Plate as one Electrode.* *By* A. P. LAURIE, *B.A., B.Sc.*[*]

IN a former number of this Journal † I have given an account of some experiments undertaken with the view of testing whether the E.M.F, of a cadmium-iodine cell was really due to the combination of cadmium and iodine. As there explained, these experiments were made on account of my feeling an increasing distrust of measurements of E.M.F. made in the usual way with infinitely small currents,—a distrust not of the measurements themselves, but of our right to assume that the E.M.F. of a particular cell was due to a particular chemical reaction. This doubt is obviously justified when once we realize that impurities far out of reach of analytic detection could easily produce sufficient current for the measurement of E.M.F. in the usual way.

On reading the papers published by Dr. Alder Wright in this Magazine in 1885, I was very much struck by the results of his experiments on aluminium-zinc cells, in which he found an E.M.F. actually opposed to that calculated from the thermal data. To quote his figures, given on p. 28, vol. xix. of the Phil. Mag. for 1885, he finds that zinc-aluminium cells, with the metals immersed in sulphate of zinc and potash-alum respectively, have an E.M.F. of ·538 volt. According to the thermal data, there should be an E.M.F. of ·982 volt in the opposite direction, so that we have a difference of 1·519 volt between the E.M.F. calculated from thermal data and that obtained by actual measurement.

Dr. Wright's experiments with other aluminium cells give similar results.

Now observe that the E.M.F. calculated from the thermal data is obtained by taking the heat of formation of sulphate of zinc and sulphate of alumina respectively, and subtracting the comparatively small heat of formation of sulphate of zinc from the large heat of formation of sulphate of alumina ; that, therefore, the assumption is made that this cell is similar to a Daniell cell, and that sulphate of alumina will be formed and sulphate of zinc decomposed, by the passage of the current.

The E.M.F. measurement, however, shows an E.M.F. in the opposite direction to that calculated on this assumption. How is this to be accounted for ? Dr. Wright accounts for it by assuming a " thermo-voltaic constant " of 1·519 volt. By this I understand him to mean that he believes that sul-

* Communicated by the Author.
† Phil. Mag. May 1886, p. 409.

phate of zinc is formed and sulphate of alumina decomposed during the passage of a current, and that an enormous absorption of heat takes place to enable this reaction to go forward.

If we believe that we are dealing only with metallic aluminium, sulphate of alumina, sulphate of zinc, and metallic zinc, then we must make the above assumption, and undoubtedly chemical analysis would confirm such a belief.

I think, then, I have said sufficient to show that we have here a point of some importance,—that if we have only the above substances to deal with, Dr. Wright has made a surprising discovery.

It was to test whether we really had these substances present that I have made the following experiments.

Before describing the experiments, however, I must say a few words about aluminium itself. It is a metal with some very peculiar and contradictory properties. Though having a very high heat of combination with oxygen, it does not tarnish in the air or decompose water at ordinary temperatures, nor is it soluble in nitric acid. If used as the electrode at which oxygen is set free by the passage of a current through dilute sulphuric acid, it opposes a very considerable resistance to the passage of the current. This resistance has been called "polarization," and stated as high as six volts.

On amalgamating the metal (which can only be done by immersing first in strong caustic) it becomes capable of decomposing water; and its oxidation in the air is so rapid that if left at rest it becomes covered with a growth of oxide in a few minutes, resembling some fungus growths in appearance.

Now all these properties seem to me to be very easily explained by supposing aluminium to become rapidly coated with a film of oxide, whether in air or water, which, being very insoluble, protects it from all further action, and practically for chemical purposes converts an aluminium plate into an aluminium-oxide plate, unless some solvent of the oxide is present.

Starting with this view, I prepared a solution of aluminium sulphate and of zinc sulphate, enclosing one in a porous pot, and immersed a zinc rod in the zinc sulphate and an aluminium wire in the aluminium sulphate. I did not note the strength of the solutions, as the experiments were merely qualitative.

I obtained a deflection on connecting this cell with the electrometer, amounting to about ·54 volt in the same direction as that obtained by Alder Wright. On removing the aluminium wire, cleaning it with sandpaper, and immediately

plunging it into the liquid and connecting with the electro-meter, a deflection was obtained in the same direction, but only amounting to ·14 volt. After ten minutes it had risen to ·35 volt. This seemed to show that the complete forma-tion of the oxide film took some time. The introduction of a little caustic potash also brought the E.M.F. nearly to zero, but I had not as yet obtained an actual reversal of the E.M.F. of the cell.

I next amalgamated the wire and plunged it in the solu-tion. I at once obtained a deflection of about ·46 volt in the *opposite direction*. At the same time hydrogen was given off, and the wire above the solution became coated with the growth of oxide already referred to.

The next experiment was an obvious one. I plunged two aluminium wires, one cleaned, the other amalgamated, in a solution of sulphate of alumina, and obtained a deflection of 1·08 volt, showing a remarkable difference in chemical pro-perties between the amalgamated and cleaned aluminium wires.

In order to test the effect of short-circuiting on the cell, I left a zinc-aluminium cell short-circuited for some days, but the aluminium plate, except for a slight darkening of the surface, remained unchanged, showing that to whatever re-action the E.M.F. of the cell was due, it was of so tempo-rary a kind, or the internal resistance of the cell was so great, that no perceptible chemical change had been brought about.

These experiments are, I think, sufficient to show that there is no evidence that the reactions in the cell are simply the formation and decomposition of the sulphates of zinc and aluminium respectively, but that there is strong evidence to show that we are not in the cell dealing with aluminium at all, but with *aluminium oxide* supported on an aluminium plate. It is therefore more probable that the E.M.F. of the cell is due to the heat of formation of zinc sulphate, — the heat of formation of aluminium sulphate, + the heat of for-mation of aluminium oxide, — the heat-formation of water. These reactions would result in an E.M.F. in the direction actually observed. It may be noticed that even the amalga-mated aluminium gives too low a value for the reactions of the formation and decomposition of the sulphates. As, how-ever, the water is being decomposed by the metal, we can hardly tell what the actual source of E.M.F. may be.

I do not know if Dr. Wright attached any definite mean-ing to the term " thermo-voltaic constant " in the case of these particular cells, as the assumption that the reactions are

the formation and decomposition of the sulphates is evidently questionable, even from the results he obtained.

This paper is not of course meant as a criticism on Dr. Wright's work, which I have always admired for its accuracy, but merely gives a new interpretation of certain of his experimental results.

XXV. *Note on a Mode of maintaining Tuning Forks by Electricity. By* Prof. S. P. THOMPSON[*].

ALL who have worked with self-maintained electric tuning-forks have met with the fact that these instruments as ordinarily arranged give a very irregular note, the pitch of which is continually slightly altering, and even those which are fairly constant in pitch are continually changing the phase of their vibrations. These changes of phase and of pitch render the electrically-sustained tuning-fork as usually constructed almost useless for acoustic work, and diminish the usefulness of the instrument for chronoscopic and electric applications. They appear to be due to a fact which is tolerably obvious to any one acquainted with the fundamental principles of the vibrations of elastic bodies, namely, that the impetus given by the electromagnet at each vibration is given at the wrong instant of the motion, namely at some other instant than that during which the fork is passing with maximum velocity through the position of zero displacement. In the older forms of electro-diapason constructed by Fessel and by Koenig[†], the electromagnet was of horseshoe form, having poles outside the prongs of the fork ; whilst in the more recent instruments by Koenig the electromagnet is of short cylindrical form, and placed between the prongs of the fork. The latter arrangement, which is preferable for several reasons electrical and mechanical, seems to have been first suggested by Lord Rayleigh[‡]. In the earlier form contact was made by a stylus, carried by the prong of the fork, dipping into the mercury cup : in the later form the stylus usually makes contact against a platinum-headed screw, almost exactly as does the interrupter of Wagner so common in electric bells. In either case contact is made at a very brief interval before the prong of the fork reaches its extreme elongation away from the pole of the electromagnet, and is broken at a slightly longer interval after the prong has passed its extreme elonga-

[*] Communicated by the Physical Society : read June 26, 1886.
[†] *Vide* Helmholtz, 'Sensations of Tone,' Ellis's edition of 1875, p. 178.
[‡] 'Theory of Sound,' vol. i. p. 56.

tion, the difference arising from the elasticity of the stylus, and the necessary imperfection of the contact until a certain actual contact-pressure has been attained. There is also a certain retardation in the electro-magnetic pull behind the instant of greatest current, owing to the self-induction of the circuit and the mutual induction between the coil and its core. But if the core be short, and laminated, and of good iron, and if the resistance of the circuit be considerable in proportion to its coefficient of self-induction, the retardation of phase in the periodic electromagnetic impulses will not be of any great importance. Lord Rayleigh remarks* that if the magnetic force depended only on the position of the fork the phase might be considered to be 180° in advance of that of the fork's own vibration. That is to say, considering a displacement of the fork toward the electromagnet positive, the maximum force occurs when the displacement is a negative maximum. But, adds Lord Rayleigh, the retardation due to self-induction and imperfect contact reduces this advance. Lord Rayleigh further remarks that if the phase-difference be reduced to 90° the force acts in the most favourable manner, and the greatest possible vibration is produced. He might have added that in this case the tendency to produce phase-change is the least possible. He suggested as a means of producing any desired retardation, the use of a stylus attached not to the prong itself, but to the further end of a light straight spring carried by the fork.

It seems to the present writer, that a better way to secure the proper timing of the impulses is to be found in the suggestion which he now makes, and which arose in his mind after considering the difference of phase which exists between two dynamo-electric machines associated together. Let two forks in unison with one another be provided, and let each act as interrupter to the other, but not to itself ; the electromagnet of each being included in the circuit of the other's contact-points. One battery will suffice for the two, as they will not both make contact at the same time.

Fig. 1 shows the proposed arrangement. The forks when started will settle down to a difference of phase corresponding to an almost exact quarter of a period.

Fork B is arranged, as shown in fig. 1, so that it makes contact at the inward stroke at the point when its displacement is at the positive maximum ; fork A makes its contact at the outward stroke when its displacement is at the negative maximum.

* *Op. cit.* p. 59.

Fig. 1.
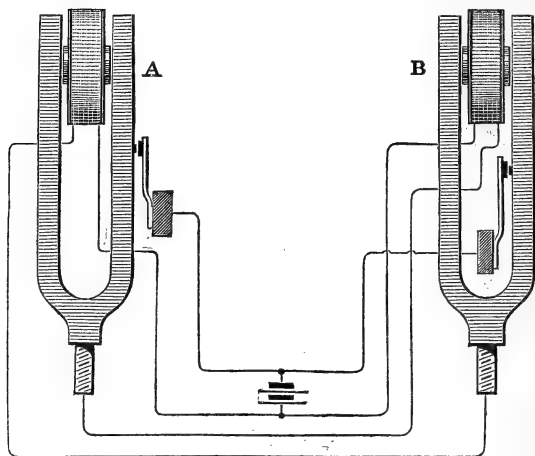
Reference to figure 2 will elucidate the phase relations. In this figure the upper curve relates to fork A and the lower to fork B. Positive values of the ordinates relate to positive displacements in the sense of approach to the electromagnet.

Fig. 2.
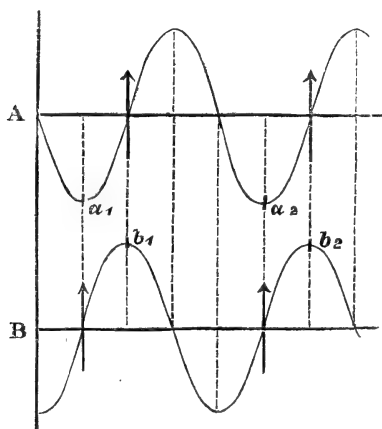
When A has moved through $\frac{1}{4}$ of its cycle of movements, and the displacement is a negative maximum a_1, it makes contact for B's electromagnet, which gives a momentary im-

pulse just as B has phase 0°. A quarter of a period later B makes contact (b_1), and gives an impulse to A just when A's phase is 0°. The arrows indicate the instant and direction of the forces. It is assumed that the retardation due to self-induction is small, as compared with the period of the forks ; and this may easily be made so, firstly, by employing only short internal electromagnets with laminated cores, and, secondly, by interposing sufficient inductionless resistance in the circuit.

Another point of some importance in the construction of electrically sustained forks is that the contacts should be very firm, and should be made much nearer the hilt of the fork than is usual in these instruments. When the stylus is near the outward end of the prong there is much greater amplitude of motion at the contact than is really requisite. A pin of platinum secured to the prong at about 5 centimetres from the point of bifurcation, making contact against a platinum-faced strip of German silver, half a millimetre in thickness, to serve as a spring, will answer the purpose for forks of ordinary size.

A common imperfection in the electro-diapason as usually constructed is the method of mounting the fork. Its shank is held with nut and washer to a block of wood or metal, which is then secured to a stand by a single bolt or screw which runs at right angles to the shank and to the planes of vibration of the prongs. The defect of this mounting is that the fork can shift a little round the bolt, and is liable to become set with one prong nearer one face of the electromagnet than the other prong is to the other face. This often results in the occurrence of actual rattling contacts between the fork and the electromagnet, as well as in derangements of the adjustment of the working contacts. It also gives rise to another kind of difficulty : if one prong is nearer to the electromagnet than the other is, there will be a tendency for the fork to vibrate as a whole around the bolt or frame upon which it is mounted, and this will give rise to slow alternations of good and bad contacts, producing on the sound an effect not unlike that of beats. Either the bolt which secures the fork to its mounting should lie in the plane of the vibrations, or else it should be replaced by a more substantial species of mounting.

XXVI. *Proceedings of Learned Societies.*

[Continued from p. 79.]

June 23, 1886.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "On some Perched Blocks and associated Phenomena." By Prof. T. McKenny Hughes, M.A., F.G.S.

The Author described certain groups of boulders which occurred on pedestals of limestone rising from 3 to 18 inches above the level of the surrounding rock. The surfaces of these pedestals were striated in the direction of the main ice-flow of the district, while the surrounding lower rock in no case bore traces of glaciation, but showed what is known as a weathered surface.

He inferred that the pedestals were portions of the rock protected by the overhanging boulder from the down-pouring rain, which had removed the surrounding exposed parts of the surface. When the pedestals attained a certain height relatively to the surrounding rock the rain would beat in under the boulder, and thus there was a natural limit to their possible height.

He referred to the action of vegetation in assisting the decomposition of the limestone, and considered that there were so many causes of different rates of waste and so many sources of error, that he distrusted any numerical estimate of the time during which the surrounding limestone had been exposed to denudation.

Considering the mode of transport of the boulders, he thought that they could not have been carried by marine currents and coast-ice, as they had all travelled, in the direction of the furrows on the rock below them, from the parent rock on the north. Moreover, marine currents would have destroyed the glaciation of the rock and filled the hollows with débris.

Furthermore, the boulders and striæ are found in the same district at such very different levels and in such positions as to preclude the possibility of their being due to icebergs.

Nor could the boulders represent the remainder of a mass of drift which had been removed by denudation, for the following reasons:—

1. They were all composed of one rock, and that invariably a rock to be found in place close by.

2. Any denudation which could have removed the clay and smaller stones of the drift would have obliterated the traces of glaciation on the surface of the rock.

3. The boulder which had protected the fine glacial markings below it from the action of the rains would certainly in some cases have preserved a portion of the stiff Boulder-clay.

4. The margin of the Boulder-clay along the flanks of Ingleborough was generally marked by lines of swallow-holes, into which

the water ran off the Boulder-clay; and when the impervious beds overlying the limestone had been cut back by denudation, a number of lines of swallow-holes marked the successive stages in the process; but there was not such evidence of the former extension of the drift up to the Norber boulders.

5. The boulders themselves were not rounded and glaciated in the same way as the masses of the same rock in the drift, but resembled the pieces now seen broken out by weathering along the outcrop of the rock close by.

Having thus shown the improbability of these boulders having been let down out of a mass of drift the finer part of which had been removed by denudation, or of their having been masses floated to their present position on shore-ice, he offered an explanation of their peculiar position, which he thought was not inconsistent with the view that they belong to some part of the age of land-ice.

That they were to be referred to some exceptional local circumstances seemed clear from the rarity of such glaciated pedestals, while boulders and other traces of glaciation were universal over that part of the country. He therefore pointed out, in explanation, that they occurred always where there was a great obstacle in the path of the ice :—at Cunswick the mass of Kendal Fell curving round at the south and across the path of the ice; at Farleton the great limestone escarpment rising abruptly from Crooklands ; at Norber the constriction of the Crummack valley near Wharfe and the great mass of Austwich grit running obliquely across its mouth. In all these cases the ice had to force its way up hill ; and there would be a time when it would just surmount the obstacle after a season of greater snowfall, and fall back after warm seasons, until it fell back altogether from that part. During the season of recession, boulders would be detached below the ice-foot ; during the seasons of advance they would be pushed forward ; and in those exceptional localities of isolated hills from which the drainage from higher ground was cut off, the boulders were left on a clean furrowed surface of limestone, which was then acted upon by rain-water and the vegetation, except where protected by the boulders.

2. " On some derived Fragments in the Longmynd and newer Archæan Rocks of Shropshire." By Dr. Charles Callaway, F.G.S.

Further evidence was added to that given in the Author's previous paper (Q.J.G.S. 1879, p. 661), to show that the Longmynd rocks of Shropshire were chiefly composed of materials derived from the Uriconian series, and that the Uriconian series itself (Newer Archæan) was partly formed from the waste of pre-existing rocks. This evidence consisted of (1) the presence, throughout the greatly developed Longmynd conglomerates and grits, of purple rhyolite fragments, recognized by microscopical characters as identical with the Uriconian rhyolites of the Wrekin, and the occurrence of grains, probably derived from the same rhyolites, in the typical green slates of the Longmynd ; and (2) the existence of conglomerate beds containing rounded fragments of granitoid rock in the core of the

Wrekin itself, whilst the Uriconian beds of other localities, and especially those of Charlton Hill, contained waterworn pebbles, chiefly metamorphic. These pebbles appeared to have been derived from metamorphic rocks of three distinct types. The views put forward were founded on microscopical evidence, of which some details were given in the paper, and were supported by the views of Prof. Bonney, who had furnished notes on the microscopical characters of the rocks.

3. " Notes on the Relations of the Lincolnshire Carstone." By A. Strahan, Esq., M.A., F.G.S.

The Lincolnshire Carstone has hitherto been supposed to be correlative with the upper part of the Speeton series, and to be quite unconformably overlain by the Red Chalk (Quart. Journ. Geol. Soc. vol. xxvi. pp. 326–347). But the overlap of the Carstone by the Red Chalk, which seemed to favour this view, is due to the northerly attenuation, which is shared by nearly all the Secondary rocks of Lincolnshire. Moreover, the Carstone rests on different members of the Tealby group, and presents a strong contrast to them in lithological character, and in being, except for the derived fauna, entirely unfossiliferous. It is composed of such materials as would result from the " washing " of the Tealby beds.

In general it is a reddish-brown grit, made up of small quartz-grains, flakes and spherical grains of iron-oxide, with rolled phosphatic nodules. Towards the south, where it is thick, the nodules are small and sporadic. Northwards, as the Carstone loses in thickness, they increase in size and abundance, so as to form a " coprolite-bed," and have yielded specimens of *Ammonites speetonensis, A. plic-omphalus, Lucina,* &c. When the Carstone finally thins out, the conglomeratic character invades the Red Chalk, similar nodules being then found in this rock.

The presence of these nodules, with Neocomian species, taken in connexion with the character of the materials of the Carstone, points to considerable erosion of the Tealby beds. On the other hand, there is a passage from the Carstone up into the Red Chalk. It would seem, then, that the Carstone should be regarded as a " basement-bed " of the Upper Cretaceous rocks.

The Lincolnshire Carstone is probably equivalent to the whole of the Hunstanton Neocomian, the impersistent clay of the latter being a very improbable representative of the Tealby Clay. It therefore follows that the whole Speeton series is absent in Norfolk, and also in Bedfordshire. The unconformity at the base of the Carstone becomes greater southwards, and the nodules have been derived from older rocks. Similarly north of Lincolnshire, where the Speeton series is overlapped, the nodules in the Red Chalk, marking the horizon of the Carstone, have been derived from oolitic rocks.

In the South of England it would seem that equivalents of the Speeton series reappear. The Atherfield clay contains an indigenous Upper Speeton fauna, while a pebble-bed near the base of the Folkestone beds is described by Mr. Meÿer as containing derived oolitic

pebbles, and being probably the representative of the Upware deposit, and presumably, therefore, also of the Lincolnshire Carstone.

4. " The Geology of Cape-Breton Island, Nova Scotia." By Edwin Gilpin, Esq., Jun., A.M., F.R.S.C., Inspector H.M's. Mines.

After referring to previously published descriptions of Cape Breton geology, the author stated that the various formations found in the island had been thus classified by the officers of the Geological Survey :—

Pre-Cambrian (Laurentian)
      including   { The Felsite series.
                       { The Crystalline Limestone series.
Lower Silurian.
Devonian.
Carboniferous, including
                         ⌠   Lower Coal-formation.
                         | { Gypsiferous series.
                        ⟨ { Limestones, &c.
                         | Millstone-Grit.
                         ⌡   Middle Coal-formation.

He then proceeded to give an account of each system and its subdivisions in order, commencing with the most ancient and adding a few detailed sections of the rocks belonging to some of the principal series. He described the distribution and relations of the several divisions.

The paper concluded with a few notes on the superficial geology of the island. There is a general absence of moraines and of the fossiliferous Post-Pliocene marine clays of the Lower St. Lawrence. The older beds are generally exposed, but deeper soils and deposits with erratic boulders are found overlying the Carboniferous beds. Marks of recent ice-action are found on the shores of some of the lakes, and are due to the ice being driven by the wind.

5. " On the Decapod Crustaceans of the Oxford Clay." By James Carter, Esq., F.G.S., &c.

6. " Some Well-sections in Middlesex." By W. Whitaker, Esq., B.A. Lond., F.G.S.

Accounts of many well-sections and borings having been received since the publication of vol. iv. of the Geological Survey Memoirs, the Author now gave more or less detailed descriptions of fifty-six of these, all in the Metropolitan county, and all either unfinished or, in a few cases, with further information as to published sections. The depths range from 59 to 700 feet, more than half being 300 feet or more deep. Nearly all pass through the Tertiary beds into the Chalk, and most have been carried some way into the latter. Papers descriptive of like sections in Essex, Herts, and Surrey have been sent to Societies in those counties.

7. " On some Cupriferous Shales in the Province of Houpeh, China." By H. M. Becher, Esq., F.G.S.

This communication contained some geological observations made during a visit to a locality on the Yangtse river, near I-chang, about 1000 miles from the sea, for the purpose of examining a spot whence copper-ore (impure oxide with some carbonate and sulphide) had been procured.

The principal formations in the neighbourhood of I-chang were said to be Palæozoic (probably Carboniferous) limestones of great thickness, overlain by brecciated calcareous conglomerate and reddish sandstones, which form low hills in the immediate vicinity of the city. About fifty miles further west the limestones pass under a great shale-series with beds of coal, the relations of which to the sandstones are not clearly ascertained.

The copper-ore examined by the writer came from the shales, which contained films and specks of malachite and chrysocolla, and in places a siliceous band containing cuprite, besides the oxydized minerals, was interstratified in the beds. Occasionally larger masses of pure copper-ore are found imbedded in the strata. The ground had not been sufficiently explored for the value of the deposits to be ascertained.

8. " The Cascade Anthracitic Coal-field of the Rocky Mountains, Canada." By W. Hamilton Merritt, Esq., F.G.S.

The coal-field named occurs in the most eastern valley of the Rocky Mountains, that of the Bow river, and, like other coal-fields of the country, consists of Cretaceous rocks, which lie in a synclinal trough at an elevation of about 4300 feet above the sea. The underlying beds, of Lower Carboniferous or, possibly, Devonian age, rise into ranges 3000 feet higher.

Further to the eastward the Jurassic and Cretaceous coal contains a large percentage of hygroscopic water and volatile combustible matter, and has the mineral composition of lignite. The average composition is:—

	Per cent.
Fixed carbon............	42
Volatile combustible matter.	34
Hygroscopic water........	16
Ash	8
	100

As the mountains are approached, the amount of hygroscopic water is found to diminish by about one per cent. for every ten miles, and fifteen miles from the range the percentage is about five. In the foot-hills the lignites pass into a true coal, with 1·63 to 6·12 per cent. of hygroscopic water, and 50 to 63 per cent. of fixed carbon. In the Cascade-river Coal-field the average character of the coal is that of a semianthracite, with the following composition :—

	Per cent.
Fixed carbon	80·93
Volatile combustible matter	10·79
Hygroscopic water	·71
Ash	7·57
	100·00

The coal-seams have been subjected to great pressure, and the change in the quality of the coal appears to be due to metamorphic influence.

9. "On a new Emydine Chelonian from the Pliocene of India." By R. Lydekker, Esq., B.A., F.G.S.

10. "On certain Eocene Formations of Western Servia." By Dr. A. B. Griffiths, F.R.S.E., F.C.S.

A great thickness of paper-shales containing paraffin occurs near the river Golabara; these extend over 30 square miles of country. Small beds of clay with rock-salt are also found: the whole is said to resemble the paraffin and salt districts of Galicia. The paraffin shale is free from bituminous impurities. It contains:—

	Per cent.
Paraffin wax	1·75
Water of combination	3·02
Ammonia	1·18

The mineral constituents of the shale are:

	Per cent.
Alumina	32·86
Iron oxide	5·20
Magnesia	1·26
Lime	1·21
Potash	2·17
Soda	0·41
Silica	56·85
Loss	0·04
	100·00

The brown coal of the neighbourhood, whose natural distillation has most probably yielded the hydrocarbon in the shales, contains:—

	Per cent.
Carbon	49·2
Hydrogen	1·1
Water, combined	30·2
Water, hygroscopic	19·5
	100·00

The beds containing these coals have been invaded by eruptive

porphyry and trachytic rocks, of which the former contains $75\frac{1}{2}$ and the latter 61 per cent. of silica.

The clays from which the shales were originally formed contain abundance of marine Diatomaceæ and Foraminifera (chiefly Nummulites), as also species of *Ostrea, Cyrena, Cerithium, Voluta,* and *Nautilus,* together with the remains of Placoid and Teleostean fishes.

XXVII. *Intelligence and Miscellaneous Articles.*

ON SOME EXPERIMENTS RELATING TO HALL'S PHENOMENON. BY PROF. BOLTZMANN.

FROM the general equations which Maxwell and Rowland had given for the movement of electricity, Lorentz (Wiedemann's *Beiblätter,* viii. p. 869) deduced the following equations for the movement of electricity in a plane plate at right angles to the lines of force of a magnetic field, provided that the Hall's deflection of the current by magnetism is taken into account :

$$u = -\kappa\frac{dp}{dx} - hv, \qquad v = -\kappa\frac{dp}{dy} + hu ;$$

u, v, p, κ are the components of the current, the electrical tension, and specific conductivity, and h a constant which is probably nearly proportional to the strength of the magnetic field. From these equations I have deduced certain conclusions which seem to me worthy of being experimentally tested. The following integral corresponds to Hall's observations :

$$p = -ax + hay, \qquad u = \kappa a, \qquad v = 0.$$

In an iron strip with the bounding lines $y=0$ and $y=b$, and the thickness δ, a current $J = \kappa ab\delta$ flows in the direction OX. The north pole is on the positive Z side. The hands of a watch the face of which is turned towards OZ, run from OX towards OY. In a Hall circuit, which from its great resistance does not materially alter the condition of the strip, a current is driven by the electromotive force $e = hab$, which flows in the iron strip, against the positive y-direction. By rotatory power R Hall understands the quotient $e\delta/\mathrm{JM}$, so that $h = \mathrm{RM}\kappa$. I may observe that the absolute values which Hall gives for R are far too small; possibly owing to a confusion of the ohm with the resistance-unity they should be multiplied by 10^9. From the above equations follows :

$$u + hv = -\kappa\frac{dp}{dx}, \qquad u = -k\left(\frac{dp}{dx} - h\frac{dp}{dy}\right),$$

$$v - hu = -\kappa\frac{dp}{dy}, \qquad v = -k\left(\frac{dp}{dy} + h\frac{dp}{dx}\right),$$

in which $k = \kappa : 1 + h^2$. The equation of continuity,

$$\frac{du}{dx} + \frac{dv}{dy} = 0,$$

gives

$$\frac{d^2p}{dx^2}+\frac{d^2p}{dy^2}=0.$$

For a circular plate it follows that when one electrode of the primary current is in the centre, and the entire periphery is the second electrode:

$p=-\mathrm{A}\log\mathrm{nat}\,r,\ \ u=k\mathrm{A}(x-hy)\,|\,r^2,\ \ v=k\mathrm{A}(y+hx)\,|\,r^2,\ \ \rho=k\mathrm{A}\,|\,r\,;$

the stream-lines are logarithmic spirals with the equation

$$\mathfrak{I}=h\log\mathrm{nat}\,r+\mathrm{const.}\,;$$

r and $\mathfrak{I}$ are the polar coordinates, ρ the components of the current in the direction of r. Similarly found stream-lines were observed in Geissler's and Hittorf's tubes under the influence of magnets. The flow in an infinite plate with two or more point-shaped electrodes is obtained by superposition. The equation of the stream-lines is then

$$h\log\mathrm{nat}\,(r\,|\,r')=\mathfrak{I}-\mathfrak{I}'+\mathrm{const.}$$

for two electrodes to which the polar coordinates r, $\mathfrak{I}$, r', $\mathfrak{I}'$ refer.

Leduc (*Journal de Physique*, 2nd series, vol. iii. p. 366) has suggested the hypothesis that the changes of resistance which magnetism produces in bismuth are merely apparent, and caused by the fact that the currents are forced into longer paths by Hall's phenomenon. In this case, from what has been above said, no changes in resistance are to be expected in a rectangular strip in the entire extent of which the currents flow parallel to two sides which lie opposite. For the circular plate the strength of the current $i=2\pi r\rho\delta=2\pi k\mathrm{A}\delta$, the resistance s is therefore

$$\frac{\log\mathrm{nat}\,(r_2:r_1)}{2\pi k\delta}=\frac{1+h^2}{2\pi\delta\kappa}\log\mathrm{nat}\,\frac{r_2}{r_1},$$

in which r_2 is the radius of the plate, r_1 that of the central electrode. Hence it would be increased in the ratio $1+h^2:1$ by the magnetism. If E is the electromotive force of the battery, p the difference of potentials of the electrodes of the plate, then

$$i=\frac{p}{s}=\frac{\mathrm{E}-p}{w}=\frac{\mathrm{E}}{s+w},$$

in which w is the resistance of the rest of the circuit. Prof. Ettingshausen found for a bismuth disk corresponding to these conditions, in two experiments with the magnetic fields 6364 and 4810, the values 1·257 and 1·180, which agree tolerably with the formula, since with bismuth from another source it is true R varied from 8 to 10, On the other hand, it seems difficult to explain the great difference in resistance which was found with rectangular bismuth strips as arising from want of homogeneity, or from the fact that the current was led, and taken away from individual points, and not at the entire breadth. The integral $p=-ax+cy$ corresponds to a rectangular strip of thickness δ, at whose shorter sides b the primary current enters and leaves, while the longer sides l are

entirely covered with numerous Hall's electrodes, each connected with the one opposite. Let E be the electromotive force of the battery which urges the primary current, J its intensity, $r = l \,|\, \kappa b \delta$ the resistance which it finds in the plate, w the remaining resistance, i the entire intensity of the Hall current, $\rho = b \,|\, \kappa l \delta$ its resistance in the plate, ω the remaining resistance, then :

$$\mathrm{E} - al = w\mathrm{J}, \quad cb = \omega i, \quad \mathrm{E} - \frac{u + vh}{\kappa} l = w\mathrm{J} = wb\delta u,$$

$$\frac{-v + hu}{\kappa} b = \omega i = \omega l \delta v, \quad \kappa \mathrm{E} \,|\, l = (1 + f)u + hv, \quad hu = (1 + \phi)v \,;$$

$$\kappa \mathrm{E} \,|\, l = \left[1 + f + \frac{h^2}{1 + \phi} \right], \quad u = \left[h + \frac{(1 + f)(1 + \phi)}{h} v \right],$$

in which $f = w \,|\, r$, $\phi = \omega \,|\, \rho$. The resistance is therefore increased in the ratio $\dfrac{h^2}{1 + \phi} : 1$, since

$$\mathrm{E} = \left[r + w + \frac{h^2 r}{1 + \phi} \right] . \mathrm{J}.$$

Sitzungsberichte der kaiserlichen Akademie in Wien, April 8, 1886.

ON THE GOLD-LEAF ELECTROSCOPE. BY FR. KOLACEK.

In the following way we get at a relation between the angle of divergence ϕ and the difference of potential P between the leaves and the envelope (base-plate metal-case). Let C be the capacity of the electroscope, considered as a condenser, which depends on ϕ; E = PC the quantity of electricity on the inner coating; l, g the length and weight of *one* of the leaves, which are assumed to be quite equal; $\mathrm{E}^2/2\mathrm{C}$ the electrostatic, and $2l/2g(1 - \cos \phi/2)$ the ordinary energy of mechanical origin corresponding to a rise of the centre of gravity of the leaves. If the electroscope has been left to itself and insulated, the variation of the entire energy $\mathrm{E}^2/2\mathrm{C} + g . l(1 - \cos \phi/2)$ equal to zero of a virtual deflection $d\phi$ is imparted to the leaves. If we consider that C, but not E, is affected by the variation, the following equation results between ϕ and P :—

$$\mathrm{P}^2 = gl \, \frac{\sin \dfrac{\phi}{2}}{\dfrac{d\mathrm{C}}{\partial \phi}}.$$

For an infinitely small ϕ, $(d\mathrm{C}/\partial \phi)\phi = 0$ is to be considered constant, and the deflection in this case indicates magnitudes, which are proportional to the square of the above difference of potential. For a greater ϕ an expansion in series must be made. The formula with two constants $a\phi + b\phi^2 = \mathrm{P}^2$ may be experimentally confirmed to a divergence of about 18°.

The leaves, 8 centim. in length, were projected in fifteenfold

magnification on a scale divided into centimetres. Notwithstanding that the images were imperfect, the error of reading was within the tenth of a division. The reading of the distance was made about twenty seconds after the first impulse; for the leaves were driven beyond the new position of equilibrium, and only attained it after some seconds. It will be seen that this difference of potential measured after twenty seconds, with equally good insulation, is a measure of this magnitude at the time of the impulse. The insulation was sufficiently good, for in 100 seconds the deflection sank for instance only from 10·3 to 8·9. In order to test the above formula, one of the poles of one, two, three Daniells was connected with a condenser-plate, the other with the earth and the second movable plate. It is clear that the charge arising from the difference of potential of 1, 2, 3 Π Daniells is again a measure for the magnitude P. Hence we have to test the formula $a\delta + b\delta^2 = \Pi^2$, if δ is the deflection determined in double centimetres. The actual distance of the leaves x, expressed in centimetres, is then $2\delta : 15$.

The condenser consisted of two copper plates, 7 centim. in diameter, in which only that screwed on the electroscope was varnished. Each experiment was repeated ten times and the mean taken. Probably owing to the formation of a residue in the shellac, the first deflections were a few percentages too small compared with the later very regular values.

Measurement gave for δ the value 1·91, 5·46, 9·48, corresponding to the value $\Pi = 1, 2, 3$ Daniells. With the latter two values of δ and Π, $a = 0·438$, $b = 0·0539$. In this case the potential-difference corresponding to $\delta = 1·91$ represents 1·016 Daniell instead of 1 Daniell.

$\delta = 5·13$ corresponds to a zinc-carbon element in potassic bichromate to which some sulphuric acid was added; hence $\Pi = 1·914$ Daniell compared with 1·86 Daniell with the electrometer.

According to the formula the deflection 17·5 represented this and the three Daniell elements, and thus $\Pi = 4·914$, which measurement gave 18·3. The unvarnished copper plate was replaced by a zinc plate of the same size cleaned with emery-paper, the metallic connection established with the condenser, and a deflection of 0·95 obtained from two distinct series of observations, each containing ten readings. With $\delta = 0·94$ the formula gives

$$\text{Zn/Cu} = 0·677 \text{ Daniell.}$$

If ϕ is the constant ratio between Π and P, and if in the above formula $\delta = 15x/2$ and $\Pi = P/\phi$, it passes into the formula

$$P = \phi \sqrt{3·825x + 3·032x^2},$$

which is independent of the magnitude and choice of the condenser. ϕ was determined by connecting the electroscope with a Beetz's dry pile of 144 elements. Of course the second pile was put to earth.

It is quite essential that the resistance of the battery be infinitely small in comparison with the resistance of the insulating parts of

the electroscope, for otherwise the entire amount of the difference of potential due to 144 elements is not produced in the electroscope.

When no current could be ascertained in the battery, even with a reflecting-galvanometer which gave ten-millionths of an ampere, the above condition seems to have been fulfilled. For if one pole was insulated, and the other connected with an electroscope, and if it was charged with an ebonite rod, an instantaneous discharge took place, when the insulated pole was put to earth.

The small divergence of the gold leaves was determined with sufficient certainty by viewing it at a distance of 1·605 metre with a powerfully magnifying spectrometer-telescope, the micrometer-screw of which was provided with a milled head divided into 100 parts, in which six parts represented a minute. As a mean of several values, the smallest of which differed from the largest by about 4 per cent, the number of divisions was found to be 12·9; so that $x = 0.1004$ centim., and thus $\phi = 240.0$. By the aid of the electroscope used and of the formula

$$P = \phi \sqrt{3\cdot285\,x + 3\cdot032\,x^2},$$

or, still better, of a curve on coordinate paper, differences of potential of considerable amount may be determined with an error amounting to perhaps 5 per cent., in which, instead of reading by projection, the far simpler method of reading by a glass micrometer in the eyepiece of the telescope may be used. The upper limit of the differences of potential to be ascertained may be determined by means of an electroscope with heavy leaves.

I might mention, in conclusion, that in the absence of a Beetz's dry pile, ϕ might be ascertained if the deflection of an electroscope connected with a Leyden jar is noted, when the jar is discharged with a given length of spark. For this the knowledge of the difference of potential corresponding to a given striking distance is required. An experiment gave a very close agreement for the value φ.—Wiedemann's *Annalen*, No. 7, 1886.

APPLICATION OF THERMODYNAMICS TO CAPILLARY PHENOMENA.
BY P. DUHEM.

Gauss and Laplace have founded a theory of capillary phenomena on molecular considerations, in which they assume that the forces with which the smallest parts act on each other possess a function of force in the ordinary mechanical sense. Poisson and several others have shown that this theory holds also when the density near the surface varies; the first supposition has been generally adopted.

This, however, is no longer admissible if we are compelled to take into account virtual displacements which changes of temperature bring about, and such changes of temperature, as Thomson has shown, are in general necessarily connected with changes of the capillary surface.

Properly to treat this subject, and to bring Thomson's investigations into connection with the older ones, we must no longer adopt the ordinary mechanical treatment, but must have recourse to general thermodynamical methods. The considerations arising from this point of view form the contents of the present research.

It is first of all shown, that for a system of bodies touching each other we are not to seek the potential at a fixed temperature, but the thermodynamical potential, which contains the changes of energy for varying temperature. The only assumption made is that the densities and the actions of the molecular forces of bodies vary only in infinitely thin layers at the surfaces.

It is seen that this supposition is sufficient to prove that the thermodynamical potential consists of two parts, one of which is a linear function of the content of the various bodies, the other a linear function of the surface in contact. This easily furnishes a proof of the admissibility of the older view. From the formulæ obtained, the laws of Gauss and Laplace for the shape of the surfaces are explicitly deduced; the same formulæ render it possible to investigate the capillary changes which occur in thermal changes. New results are thus not obtained, but the old ones are brought into connection with each other.

The general equations are then applied to the two special processes of evaporation and supersaturation.—*Ann. Ecole Normale* [3] ii. p. 217 (1885); *Beiblätter der Physik,* vol. x. p. 330.

OF PELTIER'S PHENOMENON IN LIQUIDS.
BY E. NACCARI AND A. BATTELLI.

Two glass cylinders, 16 centim. in width, were placed near each other in a vessel of water, and a paper disk fastened in each half way up. At the bottom of each cylinder was a copper disk 13 centim. in diameter, Solution of blue vitriol was poured in up to the disk, and on this solution of zinc sulphate; in each of these solutions a zinc plate perforated in the centre was suspended. Both zinc plates were connected by a copper wire and a current passed through the apparatus, the intensity of which was determined by a reflecting-galvanometer, one division of the scale of which represented an ampere. A very thin perforated glass plate was brought in the centre of each of the paper disks. In this aperture was accurately fitted the bulb of a thermometer, which was surrounded by a caoutchouc tube as far as the part in the aperture. The current of one or two Bunsen elements was sent through the apparatus in either direction, and the course of the thermometer observed every minute. If i and i_1 are the intensities of the current, y and y_1 the corresponding thermal effects, the magnitude of the Peltier phenomenon is given by the formula

$$h = (yi_1^2 - y_1 i^2)/(ii_1^2 + i_1 i^2).$$

Only those experiments were taken into consideration in which i and i_1 were not greatly different from each other. With the

following intensities were obtained :—

i	....	63	123	148	187
$10^6\, h$	....	71	64	72	67

The value of h is therefore almost independent of i, and thus Peltier's phenomenon proportional to the intensity of the current.

If when solutions of Glauber's salts of various concentration were superposed on each other, and the bulb of the thermometer was not at the surface of separation of the liquids, or if the same liquid was above and below the diaphragm, no appreciable value was obtained for h, so that the above results are not limited by other influences of heat.

From the experiments as a whole, the value of h is obtained when solutions of the following sulphates are combined with solution of $CuSO_4$ of specific gravity s, and in like manner the solutions of the chlorides with solution of cupric chloride of sp. gr. $1\cdot10$. The values of h are positive, when the greater heating effect takes place, *i. e.* when the current passes from the lower and more concentrated solution to the one above it.

Formula.	s.	h.	Formula.	s.	h.
N			$NiCl_2$	1·115	−60
$NiSO_4$...............	1·13	−49	HCl	1·029	−46
$Ni(NH_4)_2SO_4$ ...	1·07	12	$MgCl_2$	1·12	−36
$CuSO_4$	1·13	29	$ZnCl_2$	1·19	−23
$(NH_4)_2SO_4$.........	1·06	29	MCl_2	1·29	+14
Na_2SO_4	1·057	52	KCl	1·08	24
$MnSO_4$	1·1	64	NaCl	1·068	27
$MgSO_4$	1·09	74	$CaCl_2$	1·15	38
K_2SO_4	1·07	91	$MnCl_2$	1·167	38
$ZnSO_4$	1·137	101	NH_4Cl	1·026	52
$FeSO_4$	1·12	106			
H_2SO_4	1·05	120			

The series do not agree with each other. With solutions of various strengths, apart from H_2SO_4 and HCl, h is always positive.

If the entire apparatus was filled with water, and the thermometer was surrounded by a semicircle which was half of iron and half of zinc wire, and the current was passed through, the mean of ten experiments was found to be $h=18$; so that the Peltier phenomenon between liquids is not of a less order of magnitude than between metals. As according to Bellati the electromotive force between zinc and iron is $0\cdot0024$ volt, the absolute value of Peltier's phenomenon may be calculated from this for the liquids investigated.—*Atti della R. Acc. di Torino,* vol. xx. 1885; *Beiblätter der Physik,* vol. x. p. 118.

XXVIII. *On the Physical Structure of the Earth.* By HENRY HENNESSY, *F.R.S., Professor of Applied Mathematics in the Royal College of Science for Ireland.*[*]

THE structure of the Earth as a mechanical and physical question is closely connected with the origin and formation of its satellite, and of the planets and satellites belonging to the same solar system. The brilliant results obtained during the present and preceding century by the aid of mathematical analysis, whereby the motions of these bodies have been brought within the grasp of dynamical laws, may have led to the notion that by similar methods many obscure problems relating to the planet we inhabit might be accurately solved. But, although the general configuration of the Earth and planets has been treated mathematically with results which leave little to be desired, when applications of analytical methods are attempted to questions of detail in terrestrial structure, the complication of the conditions is so great as to impose the necessity on some investigators of so altering these conditions as to make their results perfectly inapplicable to the real state of the Earth. Physical Geology presents problems the solution of which undoubtedly calls for mechanical and physical considerations; but these may in general, under the complex nature of the phenomena, be often better reasoned out without the employment of the symbolical methods of analysis. In most cases the conditions are totally unlike those above alluded to, which admit of precise nume-

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[*] Communicated by the Author.

rical computations. The heterogeneous character of the rocks composing the Earth's crust, and the probably varied nature of the matter composing its interior, render mathematical applications rarely possible, and sometimes misleading. Such views seem to be gradually gaining strength among geologists who pay attention to questions of a general nature, and no one has better expressed them in recent times than Professor M. E. Wadsworth[*].

The principle upon which I have ventured to found all my researches on terrestrial physics is this: to reason on the matter composing the globe from our knowledge of the physical and mechanical properties of its materials which come under our notice. Of these properties the most important are density, compressibility, contraction or dilatation from changes of temperature. Newton and other philosophers have already adopted the same principle to a limited extent, when assuming for the mass of fluid composing the Earth in its primitive condition those specific properties which have been assigned to all kinds of fluids observed at the surface. It is impossible to frame any statement more erroneous and misleading than that I have endeavoured to render the question more hypothetical than it was. On the contrary, I have discarded the invariable assumption of mathematicians who treated the question, namely, the hypothesis of the invariability of positions of the particles composing the solidifying earth. The speculations of all rational inquirers upon the Earth's internal structure must necessarily start from the same general principle as above. Some investigators have disregarded that principle, and made the problem thereby a purely mathematical exercise.

In order to reason upon the Earth's figure, we must assume that the laws of fluid equilibrium apply to the inner portions of the fluid as well as the outer. There is nothing hypothetical in reasonings as to the formation of the solid shell and the law of increase of ellipticity of its inner surface as a result of the transition of the formerly fluid matter to the state of solidity. On the contrary, the assumptions of Mr. Hopkins and other mathematicians, that this transition created no change in the law of density of the matter composing the Earth, and in the ellipticity of the strata of equal pressure, are not merely hypothetical; they are directly opposed to well established physical and mechanical laws.

On the other hand, those who have concluded that nothing can be known of the form of the fluid nucleus, seem to deny

* "Lithological Studies." Memoirs of Harvard College Museum, vol. i. p. 3; and 'American Naturalist,' June 1884, p. 587.

that the recognized laws of matter apply to the internal condition of the Earth. The shape of the nucleus and the figures of its strata of equal density follow from physical and mechanical laws, just as the forms of the isothermal surfaces within the spheroid follow from the known laws of conduction of heat. Some of the mechanical reasonings regarding the strata of the nucleus and the structure of the solid shell can be presented without employing mathematical symbols, and in what follows I have as far as possible avoided the use of such symbols.

This course, moreover, possesses the advantage of making many parts of the reasonings more clear to geologists and observers of the stratigraphical features of the Earth, who are in reality the ultimate judges of the matter, and not mathematicians. The necessity under which the latter are constrained when dealing with problems, of throwing the preliminary propositions into simple well-defined shapes, admitting of definite deductions, obliges them to overlook the most essential conditions of the very questions at issue, and they thus arrive at results which may be precise, but which are totally inconclusive with reference to the Earth's structure.

The Mechanical and Physical Properties of the Matter composing the Earth.

(1) The materials of the Earth must manifestly influence its general structure, and no inquiries with this structure can be usefully made if the physical properties of these materials are not kept in view. If the interior of the Earth is in a fluid state it is reasonable to believe that the fluid is not the ideal substance called by mathematicians a perfect liquid, namely a substance not only endowed with perfect mobility among its particles but also absolutely incompressible. It is more reasonable to believe that the fluid in question resembles the liquid outpourings of volcanoes, or at least some real and tangible liquid whose properties have been experimentally studied. I have already shown that by overlooking this simple principle certain untenable conclusions, which assert the exclusively solid character of the Earth, have been deduced. Here I propose to develop some additional arguments relative to one of the properties of liquids which has an essential bearing upon the internal structure of the Earth.

(2) In a former paper, on the limits of hypotheses regarding the properties of matter composing the Earth's interior*, I find that, having referred to published statements where the

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* Philosophical Magazine for October 1878, p. 265.

facts were not clearly put forward, I underrated the compressibility of liquids as compared with solids. The influence of the imperfect experiments of the Academia del Cimento has long injuriously operated in defining liquid and solid matter, and has produced a remarkable conflict of opinions.

On taking the results of the best experimental investigations, it appears that, although liquids are but slightly compressible as compared with gases, they are highly compressible as compared with solids. In many treatises on Physics and Mechanics which have a high reputation matter is divided into solids, elastic fluids or gases, and incompressible fluids or liquids. Hence the erroneous inference seems to have arisen that liquids are incompressible, not only in comparison with gases, but also in comparison with solid bodies. I was surprised to find this remarkably misleading proposition formally stated long after the decisive experiments of Oersted, Colladon and Sturm, Regnault, Wertheim, and Grassi, in such a work as Pouillet's *Éléments de Physique*, and also in the German translation by Müller. The greater compressibility of liquids as compared with solids is seldom affirmed as a distinct general proposition in books on Physics. It occurs, however, in Deschanel's treatise, both in the original and in the English edition. Daguin states, in vol. i. of his *Traité de Physique*, 2nd edition, p. 40, that the compressibility of liquids was long considered doubtful, but nevertheless they are more compressible than solids.

Lamé also pointed out the great compressibility of liquids as compared to solids. I have before now referred to the statement of the same proposition in the comprehensive work of the late Professor C. F. Naumann, the *Lehrbuch der Geognosie*, vol. i. p. 269, 2nd edition[*].

Although in many physical questions the compressibility of liquids may be neglected as well as the compressibility of solids, we are not entitled to assume at any time that the latter are relatively more compressible than the former. In questions where the pressure of columns of liquid of great magnitude comes under consideration, we can no longer treat the liquid as incompressible. In the problem of oceanic tides the incompressibility of the water has been assumed, but if a planet were covered with water to a depth of one hundred miles it would be scarcely correct to make such an assumption. The compressibility is negligible in a small mass of water, but it cannot be neglected in a large mass. Such an assumption is equally unwarrantable with regard to properties

[*] " Flüssige Körper sind aber mit einer weit starkeren Compressibilitat begabt, als starre Körper."

of matter which, though negligible in some problems, are not in others. Thus in the common hydraulic questions, liquids are assumed to be incompressible ; it would be more correct to say the compressibility is neglected. In small problems connected with limited portions of the atmosphere, the compressibility of air may be also neglected, but we could not neglect it for a high column of the atmosphere. If, as before remarked, the Earth were surrounded with an ocean 100 miles deep, the compressibility of the water could not be well overlooked in tidal questions; then, *à fortiori*, compressibility cannot be neglected in such a problem as the tides of a liquid spheroid having a radius nearly equal to that of the Earth. This is immediately made manifest by expressing the compressibilities of liquids, not in terms of the amount due to a single atmosphere of pressure, as is done in most tabulated groups of results, but by some very much greater standard, such as one or two thousand atmospheres. In the experiments of Perkins[*] the highest pressure employed was two thousand atmospheres, and with this he reduced a column of water by nearly $\frac{1}{12}$ of its volume. The results of experiments with great pressures such as this are highly illustrative of the force by which a fluid may be compressed in the Earth's interior. The actual coefficients of cubical compressibility, on which calculations could be based, may be partly obtained from the more exact researches of Regnault, Grassi, and other recent experiments, or from special investigations on fluid matter conducted with precautions such as these observers have employed. By then comparing the moduli of compressibilities calculated from pressures of 1000 or 10,000 atmospheres, there could be no possibility of overlooking the consequences as to the relations of liquid and solid bodies in any case where they would be subjected to pressures of abnormal magnitude.

(3) The propagation of sound in solids and liquids gives further proof of the greater compressibility of liquids.

The rate v of transmission of sound in solids and liquids is a function of their compressibilities. In solids,

$$v = \sqrt{\frac{g\mathrm{E}}{\rho}},$$

where E is the modulus of elasticity and ρ the density. In liquids,

$$v_1 = \sqrt{\frac{g\mathrm{H}a}{\mu\rho_1}},$$

[*] Phil. Trans. 1826, p. 541.

where μ is the coefficient of cubic compressibility, H the pressure of the atmosphere, and a the density of mercury. But, as in solids, the modulus of elasticity is inversely as the compressibility k, we have

$$\frac{v}{v_1} = \sqrt{\frac{\mu\rho_1}{k\rho H a}}.$$

Both in solids and liquids the velocity of sound is inversely as the square roots of the densities and compressibilities. Although such solids as metals and rocks are denser than most liquids, the limits of their elastic compressibility are so much less, that sound is propagated far more quickly through such solids than through liquids. In steel and metals generally this has been long since established. In rocks the velocity of sound has been computed from direct experiment by Mallet, and has been found to be greater in continuous homogeneous rock than the velocities observed in liquids*.

(4) If we had not the results of direct experiment on the compressibilities of liquids and solids to assure us that these properties in liquids are in excess of those obtained for solids, we might fairly infer this conclusion from the relative dilatability of such substances under differences of temperature†. The construction of our common thermometers is based on the greatly superior dilatability of the liquids enclosed in the thermometer-tube over the material of the tube itself. The dynamical theory of heat clearly establishes that the expansion of solids and liquids is a mechanical action as much as their compression under the action of force, and the substances which contract least by cooling are precisely those which contract least under pressure. Gases which contract most by pressure are also the most dilatable by heat. Liquids occupy

* See 'Philosophical Transactions' for 1861 and 1862.

† Expansions of Metals and Glass for 1° Centigrade, according to Dulong and Petit, at different Temperatures T.

SOLIDS.								LIQUID.
Platinum.	T.	Iron.	T.	Copper.	T.	Glass.	T.	Mercury.
$\dfrac{1}{37,700}$	° 100	$\dfrac{1}{28,200}$	° 100	$\dfrac{1}{19,400}$	° 100	$\dfrac{1}{38,700}$	° 100	$\dfrac{1}{5550}$
						$\dfrac{1}{36,800}$	213	$\dfrac{1}{5425}$
$\dfrac{1}{38,300}$	311	$\dfrac{1}{22,700}$	372	$\dfrac{1}{17,700}$	328	$\dfrac{1}{32,000}$	353	$\dfrac{1}{5300}$

an intermediate place between solids and gases, in relation both to the dynamical effect of pressure and the action of loss of heat. If, instead of the experiment of the Academia del Cimento, with globes of porous metals, an experiment with equally strong but impervious vessels had been made, the deformation of each globe would have been unaccompanied by the exudation of the liquid, and the totally false statement that solids are more compressible than liquids would not have so long injuriously influenced physical science.

The Rotation of the Earth considered as partly Fluid and partly Solid.

(1) The problem of the precessional motion of the Earth, considered as a solid shell filled with liquid devoid of viscidity and friction, has been elaborately investigated by Mr. Hopkins in his 'Researches of Physical Geology' in the 'Philosophical Transactions' for 1839, 1840, and 1842, and the result obtained by him has been often quoted as extremely remarkable. Before treating the same question, it may be necessary to state that on the continent of Europe the application made by Mr. Hopkins of his result to Geology is not generally admitted, and views such as I have always firmly upheld seem to be more generally adopted ; but some confusion appears to exist as to Mr. Hopkins's results and those to which I have been led. Thus in a recent treatise on Systematic Geology, the author says, with reference to the thickness of the solid crust of the Earth, there are plainly only four possibilities to be thought of :—

1. The Earth is through and through solid.

2. The Earth is through and through fluid, with a solid crust.

3. The Earth has a solid nucleus and a solid crust, with a fluid stratum lying between.

4. The Earth is solid but furnished with cavities which are filled with fluid.

The first and last of those possibilities are not admissible, according to astronomical observations. According to the investigations of Hopkins the action exercised by the Sun and Moon on the position of the Earth's axis in space, by which Precession and Nutation are produced, would be different according to the structure we attribute to the Earth. *The values established by observation compel us to regard the Earth as for the most part in a fluid state, in order that the results may harmonize with calculation* (Pfaff, *Grundriss der Geologie*). This is the reverse of what Hopkins has concluded, and is precisely what I have long since enunciated, which I have

always continued to maintain, and which forms the cumulative result of the investigations in the text of this paper. In a Report to the Royal Irish Academy on " Experiments on the Influence of the Molecular Influence of Fluids on their Motion when in Rotation," p. 57*, I referred to a proof obtained by me of the result alluded to, and I now may be allowed to submit this proof to those interested in the question.

(2) Let us suppose the earth to consist of a solid spheroidal shell, composed of nearly similar spheroidal strata of equal density, and having the ellipticities of the inner and outer surfaces small and nearly equal. The shell is supposed to be full of liquid and to rotate around its polar axis. Under these conditions the attraction of an exterior body would tend to produce pressure between the fluid nucleus and the inner surface of the shell. Whatever may be the direction of this pressure, it can be resolved into a force normal to the shell's surface and into forces in its tangent plane. The normal force might be effective in causing a deformation of the shell, or, if the latter were rigid, it would be destroyed by the shell's resistance. If friction existed between the materials of the shell and the fluid of the nucleus, the resolved forces in the tangent plane would tend to change the motion of the shell from the motion it would have if empty. But if no friction and no adhesion existed between the particles of the liquid and the shell's nearly spherical surface, and if the particles of the liquid are free from viscidity and internal friction among themselves, this purely tangential component could exercise no influence on the motions of the shell. If the solid envelope containing fluid was bounded by planes such as a prismatic vessel or box, it is manifest that unequal normal pressures on the faces of such prism would tend to produce couples, and thus possibly rotations. Such a case has been considered by Professor Stokes, and he has shown that a rectangular prism filled with fluid will have the same motion as if the fluid was replaced by a solid having the same mass, centre of gravity, and principal axes, but with much smaller moments of inertia corresponding to these axes. But in a continuously curved and nearly spherical vessel, the normal pressure arising from disturbance of the liquid could not produce the same results. The tangential components of the forces acting at the surface of the liquid could, in this case, be alone effective, and if no friction or viscidity existed at this surface such tangential action would totally disappear. The conclusion of Mr. Hopkins's first memoir is, that if the ellipticity of the inner and outer surfaces of the solid shell were

* Proceedings of R. I. A., 2nd series, vol. iii. Science.

the same, the precession would be unaffected by the fluid, and any small inequality of nutation would be totally inappreciable to observation, p. 423, Phil. Trans. 1839. This may be rendered more manifest by recalling the general equations for the surface of a fluid obtained by Poisson, Navier, Meyer, and other mathematicians, when the internal friction of the fluid is taken into account. If a, β, γ be the angles made by the normal to the curved surface of the fluid, X, Y, Z the components parallel to the rectangular axes of $x, y,$ and $z,$ it appears that we shall have at the fluid surface, when nearly spherical,

$$\mathrm{X} = hk^2 \left[2\frac{du}{dx} \cos \boldsymbol{a} + \left(\frac{du}{dy} + \frac{dv}{dx}\right) \cos \beta + \left(\frac{du}{dz} + \frac{dv}{dx}\right) \cos \boldsymbol{\gamma} \right],$$

$$\mathrm{Y} = hk^2 \left[\left(\frac{dv}{dx} + \frac{du}{dy}\right) \cos \boldsymbol{a} + 2\frac{dv}{dy} \cos \beta + \left(\frac{dv}{dz} + \frac{dw}{dy}\right) \cos \boldsymbol{\gamma} \right],$$

$$\mathrm{Z} = hk^2 \left[\left(\frac{dw}{dx} + \frac{du}{dz}\right) \cos \boldsymbol{a} + \left(\frac{dw}{dy} + \frac{dv}{dz}\right) \cos \beta + 2\frac{dw}{dz} \cos \boldsymbol{\gamma} \right],$$

where u, v, w are components of velocity parallel to the coordinate axes, and where k is a coefficient depending on friction and viscidity. If no viscidity and no friction exists we must have $k=0,$ and hence also

$$\mathrm{X} = 0, \ \mathrm{Y} = 0, \ \mathrm{Z} = 0.$$

Now as X, Y, and Z are the effective components with which the nearly spherical mass of fluid acts at its surface when each of them is separately equal to zero, it follows that the fluid can do no work at the surface, and the motions of the shell would take place quite independently of the contained mass of fluid when the latter is totally devoid of friction and viscidity.

(3) It has long since been clearly shown that the motion of the axis of the earth, considered as a solid body, may be determined by the differential equations

$$\frac{d\psi}{dt} = -\frac{1}{\mathrm{C}n \sin \theta} \frac{d\mathrm{V}}{d\theta},$$

$$\frac{d\theta}{dt} = \frac{1}{\mathrm{C}n \sin \theta} \frac{d\mathrm{V}}{d\psi}.$$

V is the potential of the rotating solid, C its maximum moment of inertia, θ and ψ direction-angles of the axis of rotation. In the case of the Earth θ has a particular value when it becomes the obliquity of the ecliptic, and ψ the longitude of the first point of Aries. It follows that the determination of ψ and θ at any time depends upon C and V.

By analytical transformations, which are fully given by Poisson in his memoir, *Sur la rotation de la Terre autour de son centre de Gravité,* and other writers, it finally appears that the variations of θ and ψ depend on equations in which a factor enters of the form

$$\frac{2C-A-B}{C},$$

where A, B, C are the three principal moments of inertia of the earth.   In a spheroid of revolution $A = B$, and the factor becomes $\frac{2(C-A)}{C}$.   As precession depends essentially on the variation of the angle ψ, it follows that the complete expression of the factor $\frac{C-A}{C}$ is of primary importance.

(4) Mathematicians, during the past two centuries, have devoted much attention to the question of the figure of a rotating mass of fluid, with especial reference to the explanation of the spheroidal figures of the Earth and her sister planets. Solutions of this problem have been presented, especially by Clairaut, Legendre, Laplace, Gauss, Ivory, Jacobi, and Airy; and it is not a little remarkable that in applying these solutions to the case of the Earth every one of these investigators has not only supposed the Earth to have been originally in a fluid state, but that the particles of the mass retained the same positions after solidification had taken place.   This tacit or openly expressed assumption of the unchangeable position of the particles of the original fluid mass on their passage to a complete or partial state of solidity lies at the root of the whole question of the Earth's structure.   For the first time in the treatment of the physico-mathematical problem, I distinctly discarded this assumption, and I affirmed that the position of the particles of matter, on passing from the state of fluidity to solidity, must assume positions in conformity with mechanical and physical laws.   In this way the hypothesis of the Earth's primitive fluidity became more simple and much more rational ; for it was as manifestly absurd to assume that the particles of the fluid mass, on passing into a solid state of consistence, retained their original positions, as it would be to assume that if the whole Earth became liquified the positions of its particles would be unchanged.   The corrected and simplified hypothesis is also fruitful in important results; but it is singular that, as far as I am aware, no mathematician seems to have understood or appreciated its bearing on the physical structure of the Earth, except M. Plana, by a remark in a memoir published by him towards the close of his career.

(5) Before presenting my conclusions on the shape of the inner surface of the solidified shell and Plana's remark relative to the same subject, it is necessary to recall some results established by Clairaut, and frequently put forward by mathematical investigators of the Earth's figure. It seems to be universally admitted that if a mass of heterogeneous fluid composed of strata of equal density, each increasing in density from the surface of the mass to its centre, is set in rotation, the several strata will be spheroidal, but their ellipticities will not be equal. The ellipticities will decrease from the outer surface towards the centre. This law of decrease of ellipticity towards the centre is not a hypothetical result, but a necessary deduction from the properties of fluids. As all known fluids are compressible, such an arrangement of strata of equal density as that referred to must follow from the supposition of the existence of any mass of fluid of such magnitude as the whole Earth. The increase of the Earth's density from its surface to its centre is, moreover, a fact clearly revealed by the mean density of the Earth being double that of the materials composing the outside of its solid shell.

If the increase of density, in going from the surface to the centre of a large mass of fluid, is due to compression exercised by the outer upon the inner strata, it follows that the greater the total quantity of fluid the greater will be the difference between the density at its surface and its centre, and the less the quantity of fluid the less will be this difference. With a small spheroid of compressible fluid, the variation of density might be neglected, and the mass regarded as homogeneous. Suppose such a small mass of fluid to be set in rotation, its surface will become spheroidal, and it will have the well-known ellipticity $\frac{5}{4} m$, where m is the ratio of centrifugal force to gravity at the equator of the spheroid. If, now, this original spheroid be supposed to be overlaid with masses of the fluid, one after another, the inner portions will be sensibly compressed, and the whole mass will begin to vary in density in going from centre to surface. The outer surface will now present an ellipticity less than $\frac{5}{4} m$. If fresh layers of fluid are continually applied to the outer surface, the variation of density will continue, and the difference between the density at the centre and surface will increase. The ellipticity of the outer stratum of fluid will at the same time diminish to a value corresponding to the law of density. Let us now reverse this operation, and suppose a great mass of liquid in rotation, its outer stratum will be less dense than those beneath, and its greatest density must be at the centre.

Let the outer strata of equal density be successively removed, so as to leave a succession of free fluid surfaces, until a spheroid is reached in which the difference of density is insensible. It is manifest that, with each successive removal of the upper stratum of liquid, the compression in the remaining strata becomes reduced, and also the variation in density from surface to centre, until this variation becomes altogether extinguished. With the same velocity of rotation, the ellipticities of the surfaces of liquid thus successively exposed would increase up to the limiting value, $\frac{5}{4}\ m$.

If at any time of the Earth's solidification we suppose a nucleus of fluid to be enclosed within the solid shell, the successive increasing of thickness of the shell, from the congelation of the fluid matter of the nucleus, must be accompanied by the removal of successive outer strata from the nucleus. From what has been seen already, the nucleus will tend to acquire an increase of ellipticity, and therefore to mould the semifluid pasty matter about to pass into a solid state into a shape different from what it would have if no change whatever in the positions of the particles had taken place. As the nucleus is supposed to be in a state of fusion from heat, the successive additions to the inner surface of the shell from the matter of the nucleus must proceed at a very slow rate. The congelation of the surface-stratum of the nucleus must be a process of the same order of slowness as the flow of heat through the shell; and the mathematical theory of conduction established by Fourier shows that this cannot proceed otherwise than slowly. The changes in shape of the surface of the nucleus would be correspondingly slow and gradual. When once a comparatively rigid outer crust had been formed, the process of moulding additional strata of solidified matter against the inner surface of the crust from the nucleus would proceed in a slow and gradual order, so that the resulting solid strata would conform to the shape impressed upon them by the moulding forces. A remarkable illustration of the way in which fused matter ejected from the Earth's interior may, while turning on its centre and at the same time cooling, mould itself against a solid crust formed upon it has been adduced by Charles Darwin, and has been already quoted by me on a former occasion. From these considerations I have been led to conclude, *that the ellipticity of the shell's inner surface may exceed, but cannot be less than, the ellipticity of its outer surface* [*]; and, referring to the same question, Plana used the words, " La loi des ellipticités a subi dans le passage de l'état

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[*] See the subjoined representation of a section of the shell and nucleus.

liquide à l'état solide une alteration sensible par laquelle toutes les couches se sont constitueés de manière a avoir *un même* applatissement et plus grand que le précédent." M. Plana has further stated his views in the same volume of the *Astronomische Nachrichten* for 1852, thus :—" Il est permis de penser que ces couches (de la fluide intérieuse) en se consolidant, ont subi des modifications à la verité fort petites, mais assez grandes pour nous empêcher de pouvoir dériver, avec tout l'exactitude que l'on pourrait souhaiter, l'état de la Terre solide de son état antérieure de fluidité."

This paragraph gives a distinct adhesion to the improved form of the hypothesis of the original fluidity of the Earth ; and this concurrence on the part of M. Plana is the more important, as it is possible that he had formed his conclusions independently. He refers to a letter written by him on the subject to Humboldt ; and it is remarkable that, in the fifth and last volume of ' Cosmos,' published not long before the author's death, some adjacent notes allude to Plana's views, and contain references to the investigations of Mr. Hopkins and to my early researches. At this period Humboldt could scarcely have had time to examine the mechanical and physical reasonings, and he merely quoted the papers in the ' Philophical Transactions' as if he had seen them for the first time. I am not aware of any evidence as to whether Plana had known their contents ; and it is possible that his conclusions as to the forms of the strata of the shell and nucleus had been formed independently, though published a short time after my investigations.

The annexed figure may assist in making clear the results of the preceding paragraphs. The outer ellipse represents the outline of the exterior surface of the Earth's crust, which is shaded and bounded inwardly by a surface slightly more elliptical. The fluid nucleus included within the shell is represented with strata decreasing in ellipticity towards the centre. This arrangement is necessarily followed by a mass of fluid under such conditions
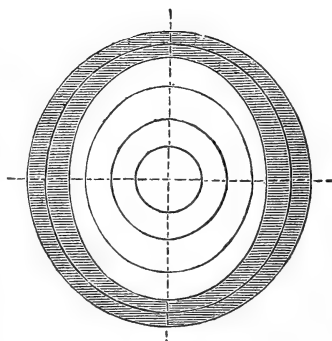
as the nucleus, or under the conditions of the entirely fluid Earth. If the matter composing the Earth underwent no change in passing from the fluid to the solid state, instead of the arrangement here represented, the inner surface of the

shell would have a smaller ellipticity than its outer surface, and the strata of the shell, as well as those of the nucleus, would be less oblate in going from the outer surface.

(6) It is important to distinctly bear in mind that the constitution of the shell and nucleus indicated by the foregoing reasonings is not based on any hypothesis of a specific law of density of the interior strata of the Earth. It is a deduction from the established properties of fluids quite as rigorous as the conclusions regarding the spheroidal shape of a mass of rotating liquid. On the other hand, the supposition tacitly or openly made by Mr. Hopkins and his followers, that the ellipticity of the inner stratum of the solid shell is precisely the same as that which this stratum had when fluid, is not merely a hypothesis—it is an assumption which is directly contradicted by the recognized physical properties of all known liquids, and even contradicted by the fundamental principles of hydrodynamics. Upon this assumption was based the calculation of the ratios of the inner and outer ellipticities of the shell which would correspond to the observed value of the precession of the Earth's axis, and hence the limiting value of the thickness of the shell. But when the fundamental assumption on which this ratio is calculated is shown to be in contradiction to physical and mechanical laws, the whole of the conclusions drawn from such a calculation must fall to the ground.

In the *Mécanique Céleste*, Laplace, following Clairaut, proved that if the density in a fluid spheroid decreases from the centre to the surface, the ellipticity of the strata of equal density must decrease from the surface towards the centre. This result forms the groundwork of some of the arguments employed in the present inquiry. Legendre and Laplace also deduced a law of density from the properties of compressible fluids, and from this law the latter unfolded a law of ellipticity of the strata of equal density. The results arrived at in my present inquiry are manifestly totally independent of the law of density $\rho = \dfrac{\mathrm{A} \sin qa}{a}$, deduced by Legendre and Laplace. In order to apply this law to the strata of the solidified shell, the assumption must necessarily be made that the particles of the fluid underwent no change in position on passing to the solid state. This was assumed by Mr. Hopkins and Archdeacon Pratt; and, as we have seen, such an assumption is not only unwarranted, but is absolutely contradicted by the established laws of hydrodynamics. My conclusions are not only in harmony with those laws, but necessarily require them to be kept constantly in view throughout the whole investigation.

(7) The result obtained in section (3) allows of an immediate and easy application to the inquiry before us, if we admit that the strata of equal density in the shell have all equal ellipticities—an admission which has been already shown to be a particular case of a rigorous and exact deduction from hydrodynamical principles. In this case let us consider the ratio of the difference of the moments of inertia of any spheroidal stratum to its greatest moment of inertia. It will readily appear that the difference of the greatest and least moments of inertia, of all the strata divided by the sum of the greatest moments of inertia will be the same as that for a homogeneous shell whose inner and outer ellipticities are equal.

If ρ be the density of any spheroidal stratum of equal density, then for that stratum

$$\frac{C_1 - A_1}{C_1} = \frac{\int \rho(x^2 + y^2)dx\,dy\,dz - \int \rho(x^2 + y^2)dx\,dy\,dz}{\int \rho(x^2 + y^2)dx\,dy\,dz},$$

and as ρ may be placed outside the sign of integration, it disappears both from numerator and denominator. As we shall presently see,

$$\frac{C_1 - A_1}{C_1} = \frac{1}{2}\left(1 - \frac{b_1^2}{a_1^2}\right),$$

where b_1 and a_1 are the semiaxes of the stratum; and for all other strata of equal density we would have

$$\frac{C_2 - A_2}{C_2} = \frac{1}{2}\left(1 - \frac{b_2^2}{a_2^2}\right),$$

$$\frac{C_3 - A_3}{C_3} = \frac{1}{2}\left(1 - \frac{b_3^2}{a_3^2}\right), \ldots \frac{C_n - A_n}{C_n} = \frac{1}{2}\left(1 - \frac{b_n^2}{a_n^2}\right).$$

Now if these strata are all similar, and have equal ellipticities,

$$\frac{b_1}{a_1} = \frac{b_2}{a_2} = \frac{b_3}{a_3} = \ldots \frac{b_n}{a_n};$$

and hence

$$\frac{C_1 - A_1}{C_1} = \frac{C_2 - A_2}{C_2} = \frac{C_3 - A_3}{A_3} = \ldots \frac{C_n - A_n}{C_n} = \frac{1}{2}\left(1 - \frac{b^2}{a^2}\right),$$

where b and a are the outer semiaxes of the shell composed of all the strata of equal density. But

$$\frac{1}{2}\left(1 - \frac{b^2}{a^2}\right) = \frac{C - A}{C} = \frac{C_1 + C_2 + \ldots + C_n - (A_1 + A_2 + \ldots + A_n)}{C_1 + C_2 + \ldots + C_n}.$$

This is the symbolical form of the proposition just stated.

In a homogeneous solid of revolution the general expression for the moment of inertia is

$$\pi \int y^2 x\, dx;$$

and from the ordinary treatises on Mechanics it readily appears that for a spheroid,

$$C = \frac{8}{15}\pi a^2 b, \quad A = B = \frac{4}{15}\pi a^2 b (a^2 + b^2),$$

where b is the semipolar and a the semiequatorial axis. Hence we have

$$\frac{C - A}{C} = \frac{2a^4 b - a^4 b - a^2 b^3}{2a^4 b} = \frac{a^4 b - a^2 b^3}{2a^4 b} = \frac{(a^2 - b^2)}{2a^4 b} a^2 b$$

$$= \frac{a^2 - b^2}{2a^2} = \frac{1}{2}\left(1 - \frac{b^2}{a^2}\right),$$

and

$$\frac{2(C - A)}{C} = \left(1 - \frac{b^2}{a^2}\right).$$

In a spheroidal shell for whose inner surface the semiaxes are b_1 and a_1, we have the moments of inertia with respect to the axes by taking the moments for the inner spheroid bounded by b_1 and a_1 from those of the outer spheroid.

Calling the former C_1 and A_1, we have, as before,

$$C_1 = \frac{8}{15}\pi a_1^4 b_1, \quad A_1 = \frac{4}{15}\pi a_1^2 b_1 (a_1^2 + b_1^2).$$

Calling C_1 and A_1 the moments of inertia of the shell, we have, therefore,

$$C_1 = \frac{8}{15}\pi(a^4 b - a_1^4 b_1), \quad A_1 = \frac{4}{15}\pi\left[a^2 b(a^2 + b^2) - a_1^2 b_1(a_1^2 + b_1^2)\right];$$

and hence

$$\frac{C_1 - A_1}{C_1} = \frac{a^2 b(a^2 - b^2) - a_1^2 b_1(a_1^2 - b_1^2)}{2(a^4 b - a_1^4 b_1)}$$

$$= \frac{a^4 b\left(1 - \frac{b^2}{a^2}\right) - a_1^4 b_1\left(1 - \frac{b_1^2}{a_1^2}\right)}{2(a^4 b - a_1^4 b_1)}.$$

If e and e_1 be the outer and inner ellipticities of the shell,

$$e = 1 - \frac{b}{a}, \quad e_1 = 1 - \frac{b_1}{a_1},$$

and if $e = e_1$,

$$\frac{b}{a} = \frac{b_1}{a_1}.$$

In this case

$$\frac{C_1 - A_1}{C^1} = \frac{(a^4 b - a_1^4 b_1)\left(1 - \frac{b^2}{a^2}\right)}{2(a^4 b - a_1^4 b_1)} = \frac{1}{2}\left(1 - \frac{b^2}{a^2}\right),$$

or

$$\frac{C_1 - A_1}{C_1} = \frac{C - A}{C}.$$

Consequently the precessional motion of such a shell would be the same as that of a homogeneous spheroid of the same ellipticity. If $e = \dfrac{1}{300}$, it appears that the value of precession for such a spheroid would be $57''$, while its observed value is $50''\cdot1$[*]. Now, as it is impossible to admit such a difference where the result of observation is so well established, we must conclude that the solid shell of the Earth, composed of nearly equielliptic strata, cannot extend to its centre—in other words, that the Earth cannot be altogether a solid from its surface to its centre. On the other hand, the fluid nucleus contained within the shell cannot be devoid of friction and viscidity, but must possess these properties in common with all fluids that have ever been observed on the Earth's surface. These properties of the liquid may, as I have long since announced, cause the shell and fluid nucleus to rotate together as one solid mass. The same conclusion has been afterwards put forward by M. Delauney ; and experiments made under his direction, and afterwards, at the instance of the Royal Irish Academy, by me, show that, in rotating glass vessels filled with water, the amount of friction and viscidity is such as to render any difference of slow motion between the liquid and its containing vessel insensible. With liquids so viscid that water is in comparison limpid, such as pitch, honey, and especially volcanic lava in a fused state, the results would be absolutely decisive. To this class of liquids the fluid matter of the Earth's interior, so far as it has come under observation, undoubtedly belongs ; and hence the overwhelming certainty of our general conclusions as to the connexion between the Earth's structure and its rotation.

(8) If the tendency of the solid crust is to become more elliptical at its inner surface as it increases in thickness, some interesting consequences appear to follow. If the shell were unaccompanied by the nucleus, or if no friction existed at their surfaces, the changes in the relations of the principal moments of inertia of the shell might be supposed to cause

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[*] A revision of the numerical data from recent astronomical results leads me to conclude that the precession for the solid spheroid would be a little less and about $55''$ instead of $57''$. This I propose to prove in a short paper, entitled "Note on the Annual Precession calculated on the hypothesis of the Earth's Solidity." This note leaves the general conclusions of the present paper unaltered.

its rotation to become unstable, so as to bring about conditions which might result in a change of the axis of rotation. It is easy to show, on the most favourable suppositions, that this could not occur. The increasing ellipticity of the inner surface of the shell would be due to the increasing oblateness of the surface of the fluid nucleus, and this would be at its maximum if the nucleus approached a state of homogeneity. But the fluid cannot approach this state unless the radius of the nucleus is so small that the variation in density due to pressure becomes insensible, whence all its strata would possess the same density. This condition, with a certain thickness of the solid shell, may bring about equality in the two principal moments of inertia of the shell. The most favourable case would be for a homogeneous shell; hence we have only to solve the very simple problem :—Given the thickness of a homogeneous spheroidal shell at its pole, required its thickness at the equator so as to make its principal moments of inertia equal. We have from the expressions for C_1 and A_1 in (7),

$$a^2 b (a^2 - b^2) = a_1^2 b_1 (a_1^2 - b_1^2),$$

or

$$a_1^4 - a_1^2 b_1^2 = \frac{a^2 b (a^2 - b^2)}{b_1},$$

which gives

$$a_1 = \frac{1}{\sqrt{2}} \sqrt{b_1^2 + \sqrt{\frac{4 a^2 b (a^2 - b^2)}{b_1} + b_1^4}}.$$

This may be written

$$\frac{a_1}{b_1} = \frac{1}{\sqrt{2}} \sqrt{1 + \sqrt{\frac{4 a^2 b (a^2 - b^2)}{b_1^5} + 1}}.$$

If we take $e = \frac{1}{293}$ for the outer ellipticity of the shell, and $e_1 = \frac{1}{230}$ for its maximum inner ellipticity, we can easily find the values of $\frac{a}{b}$ and $\frac{a_1}{b_1}$; from whence it appears that in order to have equal moments of inertia the thickness of the shell should be ·047 of its equatorial semiaxis, and the mean radius of the nucleus would thus be reduced from the original value when the whole mass was fluid by a fraction less than one twentieth. Under these conditions the ellipticity of $\frac{1}{230}$, corresponding to homogeneity, could not exist; and hence it may be concluded that, whether the shell is thin, or whether the

Earth has become almost altogether solid, the moment of inertia of the shell with respect to its polar axis must be always greater than the moment of inertia for its equatorial axis.

The tendency of the fluid nucleus to increase in ellipticity might produce a result worthy of examination by volcanologists, namely, a possible increase in the development of volcanic phenomena in equatorial as compared to polar regions with the progressive solidification of the Earth up to a certain point. Until the thickness of the shell has become very great, recent periods should exhibit a greater development of volcanic energy towards the equator than towards the poles, as compared to remote epochs.

XXIX. *On the Magnetic Torsion of Iron and Nickel Wires.*
By SHELFORD BIDWELL, *M.A., F.R.S.*[*]

IN a paper published in the Phil. Mag. for July 1886, p. 50, Prof. G. Wiedemann refers to his well-known experimental researches into the relations between the torsion and magnetization of iron wires. Let a longitudinally magnetized wire NS, be fixed at the south end S, the other end N being free, and let a battery current be passed through it from S to N, then the free end will be observed to twist in the direction of the hands of a clock as seen from the fixed end.

Maxwell [†] explains the phenomenon thus. The wire being magnetized both circularly and longitudinally, the resultant magnetization is in the direction of a right-handed screw round the wire. Now Joule found that an iron bar when longitudinally magnetized was increased in length, its transverse dimensions being at the same time contracted. We should expect therefore that a spirally magnetized wire would expand in the direction of the magnetization and contract in directions at right angles to the magnetization. And thus the twisting would be produced.

This explanation assumes that the torsional effect is simply secondary to and dependent upon the phenomena observed by Joule, and Prof. Wiedemann, for reasons given in his paper, appears not to be satisfied with it.

Some additional light may perhaps be thrown on the subject by a few experiments which I made at the beginning of the present year. They were intended to be merely of a

* Communicated by the Author.
† ' Electricity,' ii. § 448.

preliminary nature, and I had hoped to be able to carry them further; but so far as they go they appear to be conclusive, and a short account of them may be of interest at the present time in connection with the recent publication of Prof. Wiedmann.

In two papers communicated to the Royal Society [*], I have pointed out that the effect of magnetization upon the dimensions of an iron rod is not so simple as it had previously been believed to be. Joule enuciated the law that the elongation in a given bar is at first proportional to the magnetic intensity, and that it ceases to increase after the iron is fully "saturated." My own experiments show that if the magnetization is carried beyond the point at which the elongation reaches a maximum, the length of the rod, instead of remaining unchanged, steadily diminishes, until, when the magnetizing current has attained a certain strength, the original length of the rod is unaffected, and if this strength be exceeded actual *retraction* is produced. From some further experiments, details of which are not yet published, it appears that effects of the same character occur when rings are used instead of straight rods. The diameter of an iron ring surrounded by a coil of wire was found to be increased when a comparatively small current was passed through the coil and diminished when the current was strong.

I have also carried further Joule's experiments regarding the effects of magnetization upon the length of iron wires under tension and ascertained, among other things, that a wire when stretched by a weight attains its maximum elongation with a smaller external magnetizing force than when it is free, retraction apparently beginning at an earlier stage of magnetization.

Lastly, I have repeated and confirmed the experiments of Barrett [†] upon nickel. Barrett discovered that the length of a nickel bar when longitudinally magnetized was diminished. So far as appears from his published papers on the subject, which are very brief, he worked only with comparatively strong magnetizing currents, and it seemed to me possible that, as in the case of iron, weaker currents might cause elongation. But on trying the experiment I found that this was not so. Magnetizing forces which were so small as to produce no sensible effect whatever upon iron caused considerable diminution in the length of the nickel bar, and the curve of retraction given in my paper (p. 131) clearly passes through the intersection of the axes, and shows quite con-

[*] Proc. Roy. Soc. xl. pp. 109, 257.

[†] 'Nature,' xxvi. 585, and Brit. Assoc. Rep. 1882.

clusively that elongation cannot occur at any early stage of the magnetization.

Here then we have three classes of phenomena, which if Maxwell's explanation were correct would enable us to predict certain variations in the torsional effects observed by Wiedemann.

Let us consider first the case of nickel. According to Maxwell, a spirally magnetized iron wire is twisted in a certain manner, because the iron expands in the direction of the magnetization. Since, then, nickel contracts in the direction of the magnetization, we should expect a nickel wire to twist oppositely to an iron wire under similar conditions. This I found to be so. Whether the magnetizing currents were strong or weak, the twist of the nickel wire was always in the direction opposite to that given by Wiedemann for iron.

I was not aware until I read Wiedemann's recent paper that this experiment had been performed previously *. Wiedemann discusses it fully and admits that the facts accord with Maxwell's explanation (p. 54). But he appears to consider that it is a case of merely accidental coincidence : it is true that when longitudinally magnetized, iron expands while nickel contracts, and it is true that, when spirally magnetized, iron and nickel twist oppositely ; but the two sets of phenomena are quite independent and are not related to one another as cause and effect. The torsion is to be explained, he thinks, by supposing that "the obliquely spiral direction which the molecules take up in consequence of the two magnetizations at right angles to each other " is, owing to intermolecular friction, " accompanied by a displacement of the longitudinal fibres and [of the] sections of the wires." In iron the friction of the longitudinal fibres is the greatest, and thus the torsion in the observed direction is accounted for. In nickel the friction of the sections predominates ; a nickel wire is therefore twisted oppositely to an iron wire. For a more complete statement of Wiedemann's views reference must be made to the paper †.

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* Knott, Proc. Roy. Soc. Edin. 1882–3. Quoted by Wiedemann.

† I confess that I do not find it easy to follow the suggested explanation. Passing over preliminary difficulties, and assuming that the friction between the polar ends of adjoining molecules is different from that between their sides, it seems to me that the observed torsions could only be accounted for by assuming that in iron the friction of the ends is greater than the lateral friction, while in nickel the lateral friction is greatest. This appears to be directly opposed to Wiedemann's statement ; but though I have considered the question carefully and from several points of view, I can arrive at no other conclusion. It is, however, possible that I may have altogether misunderstood the argument, the more so as the paper referred to is a translation.

The behaviour of nickel then, though in agreement with Maxwell's hypothesis, is not accepted by Wiedemann as affording confirmation of it.

But we have further means of testing the hypothesis. It has been said that an iron rod when very strongly magnetized is contracted instead of being elongated. Moreover, when the rod is stretched by a weight contraction occurs with smaller magnetizing forces than when it is unstretched. In accordance with Maxwell's explanation, therefore, an iron wire spirally magnetized by very strong currents should twist (just as if it were a nickel wire) in the opposite direction to that indicated by Wiedemann for iron ; and this reversal of the twist should take place at an earlier stage of the magnetization if the wire were stretched. Now this is exactly what I have found to be the case. The free end of an iron wire, through which a constant current is passing, can be made to twist in either direction by varying the current through the surrounding helix, while with a certain medium strength of current there is no movement at all. And again, when the wire is loaded with a weight, the current which produces no torsion is considerably diminished, the reversal of the torsion also, of course, occurring with smaller currents.

It is not easy to see how Wiedemann's theory could fairly be made to explain these phenomena. He would be compelled to assume that when the iron is more intensely magnetized, that is, when the molecules are more completely turned in the same direction, the excess of longitudinal over transverse molecular friction, instead of becoming more marked, as might naturally be expected, would be decreased, and, with a sufficient degree of magnetization, even converted into a deficiency. It would also be necessary to assume that when the molecules of a wire are drawn apart by stretching the friction between their ends is nevertheless greater. Such assumptions would, I think, be highly unscientific.

On the other hand, Maxwell's explanation, which does not seek to go behind the magnetic elongations and retractions detected by Joule and others, fits the newly observed facts easily and naturally.

Note on the Experiments.

It seems desirable to give a few details of the experiments above referred to, though too much importance must not be attached to the quantitative results :—

The iron wire used was ·7 mm. in diameter and 20 cm. long. It was suspended in an upright position from a fixed clamp, and passed through a helix consisting of 876 turns of copper wire in

12 layers. To the lower end of the suspended wire was fixed a small plane mirror which reflected an indicating beam of light upon an ordinary galvanometer-scale 127 cm. distant from it. Each scale-division was equal to ·64 mm. A steady current of about 1 ampere was passed through the iron wire from the clamp to the free end, which dipped into a mercury-cup. The magnetizing coil was in circuit with a battery of 7 Grove cells, a box of resistance coils, a tangent galvanometer, and a contact key.

The deflections of the spot of light when various currents were passed through the coil are given below :—

Iron wire unstretched.

Current.	Deflection.
0·36 amperes.	+7 divisions.
1·2 ,,	0 ,,
3·7 ,,	−4 ,,

Same wire loaded with 6·3 kilos.

0·33 amperes.	+2 divisions.
0·59 ,,	0 ,,
3·68 ,,	−3 ,,

The current which produced no deflection was obtained tentatively by varying the resistance ; a current slightly stronger or weaker than that indicated caused a perceptible deflection in one direction or the other. It will be seen that when the wire was loaded with a weight of 6·3 kilogrammes the current causing no deflection was reduced to one half of its former value. The deflections given above are those produced by the second and subsequent currents of the strength denoted. That due to the first current was uncertain, depending probably upon the previously existing permanent magnetism of the wire. This point, amongst others, requires further investigation.

XXX. *Tests of* Herschel's *Æthereal Physics.*
By PLINY EARLE CHASE, *LL.D.**

JEVONS says (' Principles of Science,' ii. p. 145) :—" Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point of space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the Earth's surface, so that the pressure of the æther upon a square inch of surface must be about 17,000,000,000,000, or seventeen billions of pounds ; yet we live and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis ; yet

* Communicated by the Author.

they are no more than the observed phenomena of light and heat force us to accept."

Fiske ('The Unseen World,' p. 20) cites the numerical portion of the above statement, and explains, in a footnote, that "The figures, which in the English system of numeration read as seventeen billions, would in the American system read as seventeen trillions."

Prof. De Volson Wood (Phil. Mag. [5] xx. p. 390) says :— "Computations have been made of the density, and also of the elasticity, of the æther, founded on the most arbitrary, and in some cases the most extravagant, hypotheses. Thus Herschel estimated the stress (elasticity) to exceed

$$17 \times 10^9 = (17,000,000,000) \text{ pounds per square inch};$$

and this high authority has doubtless caused it to be widely accepted as approximately correct. But his analysis was founded upon the *assumption* that the density of the æther was the same as that of air at sea-level, which is not only arbitrary, but so contrary to what we should expect from its non-resisting qualities, as to leave his conclusion of no value. That author also erred in assuming that the tensions of gases were as the wave-velocities in each, instead of the mean square of the velocity of the molecules of a self-agitated gas ; but this is unimportant, as it happens to be a matter of quality rather than of quantity. Herschel adds, 'considered according to any hypothesis, it is impossible to escape the conclusion that the æther is under great stress.'"

The mistakes which I will endeavour to point out in the foregoing paragraphs seem to have widely prevailed. They are the more remarkable on account of the especial pains which were taken by Herschel to guard against them, and on account of some of the results, which Prof. Wood supposes to be new, being identical with those of Herschel and having been obtained by precisely the same methods.

Herschel discussed the question of ætherial intensity, first, on the corpuscular ('Familiar Lectures,' pp. 271–4), secondly on the undulatory hypothesis (ibid. pp. 274–82) ; the second portion of his discussion being made fuller and more satisfactory, on account of the present general adoption of the theory on which it rests. His procedure was substantially as follows :—

Let h = height of Earth's homogeneous atmosphere ;
  v_s = velocity of sound ;
  v_λ = velocity of light ;
  g = gravitating acceleration at Earth's equatorial surface ;

$e_1, d_1 =$ elasticity, density, of air ;

$e_2, d_2 =$ elasticity, density, of æther ;

$\quad v_s = \sqrt{gh} = 916$ ft. ;

$\quad v_\lambda = 186,000$ miles ;

$$\frac{e_1}{d_1} : \frac{e_2}{d_2} : : v_s^2 : v_\lambda^2 : : 1 : 1,148,000,000,000,000. \quad . \quad . \quad (1)$$

Accordingly, he says :—" Let us suppose now that an amount of our ætherial medium equal *in quantity of matter* to that which is contained in a cubic inch of air (which *weighs* about one third of a grain) were enclosed in a cube of an inch in the side. The *bursting-power* of air so enclosed we know to be 15 lb. on each side of the cube. That of the imprisoned æther, then, would be 15 times the above immense number (or upwards of 17 billions) of pounds. Do what we will, adopt what hypotheses we please, there is no escape, in dealing of the phenomena of light, from these gigantic numbers ; or from the conception of *enormous physical force in perpetual exertion at every point, through all the immensity of space.*"

The italics in the above extract, which are Herschel's, show that the " bursting-power " referred to is not that of simple ætherial elasticity, *assumed* to be " the same as that of air at sea-level," but is that which is represented by the ratio of the elasticity to the density, or that which would be exerted if the air and the æther were reduced to the same density.

The identity of Herschel's and Wood's methods is shown by the following extract[*] :—" It may be asked, Can the kinetic theory, which is applicable to gases in which waves are propagated by a to-and-fro motion of the particles, be applicable to a medium in which the particles have a transverse movement, whether rectilinear, circular, elliptical, or irregular ? In favour of such an application it may be stated that the general formulæ of analysis by which wave-motion in general, and refraction, reflection, and polarization in particular, are discussed, are fundamentally the same ; and in the establishment of the equations the only hypothesis in regard to the path of a particle is—It will move along the path of least resistance. The expression $V^2 \propto e \div \delta$ is generally true for all elastic media, regardless of the path of the individual molecules. Indeed, granting the molecular constitution of the æther, is it not probable that the Kinetic theory applies more rigidly to the æther than to the most perfect of the known gases ? " The identity of results is shown by Prof. Wood's statement (*l. c.* p. 416) that " In a pound of the æther there is some 100,000,000,000 times the Kinetic energy of a

* Phil. Mag. *l. c.* p. 392.

pound of air." One cipher, however, has been omitted here, and three ciphers were omitted in his reference to Herschel's estimate of ætherial stress (*l. c.* p. 390).

The following paragraph (*l. c.* p. 415) furnishes another evidence:—

" The ratio of the elasticity to the density in the æther is exceedingly large compared with the same ratio in air. The temperature of air being taken at 60° F. and the æther at 20° F., absolute, the ratio is, with sufficient accuracy,

$$\left(\frac{980,000,000}{1090}\right)^2 = 8 \times 10^{11} \text{ nearly.}"$$

Herschel's result, calculated upon the same basis (*op. cit.* p. 282), was 811,801,000,000. Through an inadvertent transposition, Maxwell (*Enc. Britan.*, English edition, article " Ether ") gave 842·8, instead of 482·8, for the quotient of 30·176 by $\frac{1}{16}$, thence deducing for the ætherial density $9·36 \times 10^{-19}$. Substituting in (1) the corrected value of $d_2 \div d_1$ (5·32 $\times 10^{-19}$), we get, for Herschel's estimate, $e_2 : e_1 :: 1 : 1,636,750$. This represents an ætherial elasticity, or " bursting-power," of about $\frac{1}{6820}$ ounce on each side of a cubic inch, instead of " upwards of seventeen billions of pounds." The weight of the cubic inch of æther would be only $\frac{1}{2.467.500,000,000,000,000,000}$ ounce.

The doctrine of conservation of angular velocity lends importance to the following additional test :—

$v_\lambda = 186,000$ miles = velocity of light ;

$t_\lambda = 498·99$ sec. = time in which light traverses Earth's semi-axis major, according to Nyren's constant of aberration ;

$\rho = v_\lambda \times t_\lambda = 92,812,000$ miles = Sun's mean distance ;

$n = 214·4513 = \rho \div r_0$;

$r_0 = 432,790$ miles = Sun's semi-diameter ;

$r_3 = 3962·8$ miles = Earth's semi-diameter ;

$\sqrt{g_a \rho} = 2\pi\rho \div 31,558,149 = 18·479$ miles = Earth's mean orbital velocity ;

$g_a = ·0000036791$ mile = Sun's mean gravitating acceleration in Earth's orbit ;

$g_0 = n^2 g_a = ·1692$ mile = gravitating acceleration at Sun's surface ;

$g_3 = ·006772$ mile = gravitating acceleration at Earth's equator ;

$t_0 = 1,099,291$ sec. $= 12·7233$ deg. $= v_\lambda \div g_0$ = time of solar half rotation ;

$m_0 =$ Sun's mass ; $m_3 =$ Earth's mass.

$$m_0 : m_3 :: g_0 r_0^2 : g_3 r_3^2 :: 332,077 : 1.$$

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XXXI. *Bars and Wires of varying Elasticity.*
By C. CHREE, *B.A., Fellow of King's College, Cambridge*[*].

IN a previous paper[†] I considered some cases of bars and
wires whose elastic properties, though constant over a
cross section, varied from point to point of their length.   In
the present paper a similar treatment is applied to certain
cases where the elastic properties vary with the distance from
the centre of the circular cross section, but are independent
of the distance of the cross section from the ends.   Before
entering on this question, however, I wish to make some
remarks on the trustworthiness of the method employed in my
last and in the present paper.

The mathematical determination of stress as a function of
strain is based upon a consideration of the molecular forces
operating at points in the interior of a solid.   A difference
of opinion as to the nature of these forces has separated
elasticians into two schools, who differ as to the number of
independent elastic constants.   Both schools, however, appear
to agree in assuming the same relation between stress and
strain to hold at the surface of a solid body as in its interior.
On this assumption, generally tacitly made, are based the
ordinary surface-equations whose validity seems to have been
seldom questioned.

The phenomena existing at the common surface of two
distinct elastic solid media have not been considered by many
mathematicians.   Green, however, in his celebrated papers on
light, and others have supposed the same relations between
stress and strain to exist in the surface-layers of each medium
as in its interior, and this was assumed without comment in
my previous paper.

It is, however, apparent, that there will be in, say the first
medium, a surface-layer of molecules within the range of
molecular action of the molecules of the second medium.
Thus, when one of these molecules in the first medium is dis-
placed, the change in the molecular force acting on it will be
due partly to the action of the molecules of that medium, and
partly to the action of those of the second medium.   It is
thus by no means obvious that the resultant change of mole-
cular force (*i. e.* the stress) will depend only on the elastic
constants of the first medium.   We may in fact imagine two
surfaces constructed, the one in the first medium and the
other in the second, between which the molecules are sub-
jected to forces which have their seat in both media ; and to
this, extremely limited, region the strict applicability of the

ordinarily accepted equations seems to me liable at least to criticism.

Since, however, the range of molecular action, and thus the distance between these two imaginary surfaces, must be extremely small, the elastic stresses over these surfaces must form equilibrating systems ; and these systems of stresses are, without any assumption, each expressible by the ordinary formulæ. It would thus appear that the ordinarily accepted equations between the stresses at the common surface of two media are, on the whole, as trustworthy as the three equations usually given for the stresses at the surface of a single medium.

In the case of media of finite thickness, the equations I have applied would thus appear as reliable as the ordinary equations for a single medium ; but their extension to a continuously varying medium is open to objection. If, however, as in the cases worked out in my previous paper and in the present, the change be so gradual as to be inappreciable at distances of the same order as the range of molecular action, the results obtained should be, at least for practical purposes, comparatively satisfactory.

If, as in the present paper, the surfaces of separation be not plane, but cylindrical, no fresh difficulty is introduced, as the range of molecular action may be regarded as vanishing compared to the radii of curvature.

There are a great variety of structures whose elastic properties vary with the distance from a central axis, though the same at corresponding points in all cross sections. Such a condition of matters is sometimes intentionally produced ; as when a bar, solid or hollow, is protected from the action of certain fluids or gases by a coating of some other material. The effect may also be due to the gradual operation of natural agents, as when girders are exposed to the action of air or water. Sometimes the process of manufacture involves such variation in the material. Thus the process of drawing increases the density and tenacity of the surface portions of a wire, which form a sort of rind to the inner and softer portions. Again, in metal casts of large cross section, it is well known that the surface-portions differ very markedly from the interior, and that in particular the strength does not by any means increase as fast as the cross section.

It thus seems desirable to find a solution for the equilibrium of a cylindrical bar exposed to longitudinal traction or pressure, or to torsion, the bar being composed of two or more materials, or of one continuously varying material, such that surfaces of equal elasticity are cylinders coaxial with the outer and inner surfaces of the bar.

For this end the fundamental equations of my previous paper will suffice, and a reference to them will be denoted by the letter a attached to the number of the equation. The notation of that paper is also followed in the present.

Let us, then, suppose a straight bar, of length l and uniform cross section, to be composed of two different materials ; the elastic constants of the inner material being m, n, and those of the outer m_1, n_1. For greater generality we may suppose the bar to be hollow, its inner surface being the cylinder $r = b$; the surface of separation and the outer surface are coaxial cylinders whose radii are a_1 and a respectively.

From (4a), (12a), (13a), and (14a) we see that suitable solutions are, in the case of longitudinal traction or pressure, for the inner material,

$$\left.\begin{aligned}
\delta &= 2A + B, \\
u &= Ar + \frac{A'}{r} \\
w &= Bz \ ;
\end{aligned}\right\} \quad , \quad \cdots \cdots \quad (1)$$

and for the outer,

$$\left.\begin{aligned}
\delta_1 &= 2A_1 + B, \\
u_1 &= A_1 r + \frac{A_1'}{r}, \\
w_1 &= Bz \ ;
\end{aligned}\right\} \quad \cdots \cdots \quad (2)$$

where B, A, A$'$, A$_1$, A$_1'$ are constants to be determined from the surface-conditions. We shall suppose the bar fixed at the end $z = 0$ so that w and w_1 both must vanish with z. It is then obvious that if the materials are to stick together throughout, B must be the same in the expressions for w and for w_1. At each of the three surfaces $r = b$, $r = a_1$, $r = a$ we have to satisfy the equations (10 a), putting $\lambda = 1$ and $\mu = \nu = 0$. Now the only stresses corresponding to the solution (1) are

$$P = (m-n)\delta + 2n \frac{du}{dr} = (m-n)B + 2mA - \frac{2n}{r^2} A', \quad . \quad (3)$$

$$R = (m-n)\delta + 2n \frac{dw}{dz} = (m+n)B + 2(m-n)A \ ; \quad . \quad (4)$$

and to the solution (2) in like manner,

$$P_1 = (m_1 - n_1)B + 2m_1 A_1 - \frac{2n_1}{r^2} A_1', \quad \cdots \qquad . \quad (5)$$

$$R_1 = (m_1 + n_1)B + 2(m_1 - n_1)A_1. \quad \cdots \cdots \cdots \quad (6)$$

Thus the equations (10 a) give, neglecting any surface normal forces such as atmospheric pressure, which can, how-

ever, if desired, be easily considered separately,

when $r=b$, $P=0$, *i. e.*

$$(m-n)B + 2mA - \frac{2n}{b^2}A' = 0, \quad . \quad . \quad . \quad (7)$$

when $r=a$, $P_1=0$, *i. e.*

$$(m_1-n_1)B + 2m_1A_1 - \frac{2n_1}{a^2}A_1' = 0; \quad . \quad (8)$$

and when $r=a_1$,

$$(m-n)B + 2mA - \frac{2n}{a_1^2}A' = (m_1-n_1)B + 2m_1A_1 - \frac{2n_1}{a_1^2}A_1'. \quad (9)$$

At this last surface also the radial displacements in the two media must be equal; therefore

$$Aa_1 + \frac{A'}{a_1} = A_1 a_1 + \frac{A_1'}{a_1}. \quad . \quad . \quad . \quad . \quad (10)$$

From (10),

$$A_1' - A' = a_1^2(A - A_1);$$

while from (7), (8), and (9),

$$n_1 b^2(a^2 - a_1^2)A_1' + na^2(a_1^2 - b^2)A' = 0.$$

Thus, if for shortness,

$$n_1 b^2(a^2 - a_1^2) + na^2(a_1^2 - b^2) = \frac{1}{\Delta_1}, \quad . \quad . \quad (11)$$

we get

$$\left.\begin{array}{l} A' = -n_1 a_1^2 b^2(a^2 - a_1^2)\Delta_1(A - A_1), \\ A_1' = na_1^2 a^2(a_1^2 - b^2)\ \Delta_1(A - A_1). \end{array}\right\} \quad . \quad . \quad (12)$$

If, again, for shortness,

$$\frac{1}{\Delta_1} + \frac{n n_1}{m m_1}a_1^2\{m_1(a^2 - a_1^2) + m(a_1^2 - b^2)\} = \frac{1}{\Delta_2}, \quad . \quad (13)$$

we get by substituting the values (12) in (7) and (8), dividing these equations by $2m$ and $2m_1$ respectively, and subtracting,

$$A - A_1 = B(\sigma_1 - \sigma)\frac{\Delta_2}{\Delta_1}, \quad . \quad . \quad . \quad . \quad (14)$$

where

$$\sigma = \frac{m-n}{2m}, \quad \sigma_1 = \frac{m_1 - n_1}{2m_1}.$$

Thus from (7) and (8), finally, we obtain

$$\left.\begin{array}{l} A = -B\left\{\sigma + (\sigma_1 - \sigma)\dfrac{n n_1}{m}a_1^2(a^2 - a_1^2)\Delta_2\right\}, \\ A_1 = -B\left\{\sigma_1 - (\sigma_1 - \sigma)\dfrac{n n_1}{m_1}a_1^2(a_1^2 - b^2)\Delta_2\right\} \end{array}\right\} \cdot \quad (15)$$

Also from (12) and (14),

$$A' = -B(\sigma_1-\sigma)n_1a_1^2b^2(a^2-a_1^2)\Delta_2, \\ A_1' = B(\sigma_1-\sigma)na_1^2a^2(a_1^2-b^2)\Delta_2. \Bigg\} \quad . \quad (16)$$

If F denote the total amount of traction, or negative pressure, applied over the end $z=l$ of the bar, we can determine B from the condition

$$F = \pi\{(a_1^2-b^2)R + (a^2-a_1^2)R_1\}.$$

Referring to (4) and (6), using (15), and denoting $\frac{n}{m}(3m-n)$ by M, and $\frac{n_1}{m_1}(3m_1-n_1)$ by M_1, we find very simply

$$F = \pi B[M(a_1^2-b^2) + M_1(a^2-a_1^2) \\ + 4(\sigma_1-\sigma)^2 n\, n_1 a_1^2(a^2-a_1^2)(a_1^2-b^2)\Delta_2]; \quad (17)$$

which determines B in terms of given quantities.

It should be noticed that, strictly, F should be distributed so that there should be at every point of the terminal section in the one material the traction R, and in the other the traction R_1. In any practical case this is not likely actually to occur; but if the traction be applied symmetrically so that its resultant acts along the axis of the beam, and if the radius of the cross section be small compared to the length of the beam, the above solution may be regarded as sufficiently correct, except perhaps for points close to the terminal section.

From (15), (16), and (17) all the constants of the solutions (1) and (2) are expressed directly in terms of F and other given quantities; thus these solutions are complete.

If, as in the case of ordinary wire, the cylinder be solid to the centre, we have $b=0$, and so

$$\frac{1}{\Delta_1} = na^2a_1^2,$$

and

$$\frac{1}{\Delta_2} = na_1^2\left[a^2 + \frac{n_1}{m\,m_1}\{m_1(a^2-a_1^2)+ma_1^2\}\right].$$

Thus the expressions (15) are comparatively short; while from (16) $A'=0$, and A_1' is a simple expression.

In the event of σ_1 and σ being equal a great simplification occurs. From (16) we see that A' and A_1' identically vanish, while from (15),

$$A = -B\sigma, \quad A_1 = -B\sigma_1 = A.$$

Also from (17),

$$F = \pi B[M(a_1^2-b^2) + M_1(a^2-a_1^2)]. \quad . \quad . \quad (18)$$

Thus the solutions (1) and (2) coincide, both reducing to

$$\left.\begin{array}{l} u = -\sigma\mathrm{B}r, \\ w = \mathrm{B}z\,; \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where **B** is simply determined by (18).

According to many foreign elasticians, following Poisson, $m = 2n$ for any elastic solid ; if this were correct, then necessarily $\sigma = \sigma_1$, and the simple solution (19) would be true for any combination of two elastic materials forming a cylinder, whether hollow or not. This theory, however, seems to be contradicted by experiment ; still if, as in the case of wire, both materials are of the same metal but have been exposed to different treatment, it seems by no means unlikely that the variation of the elastic constants m and n will follow the same law, in which case $\sigma = \sigma_1$ obviously. In actual wire, of course, there is no strict surface of demarcation answering to the cylinder $r = a_1$ of the above problem ; but in many cases the transition is very rapid, and the absolute amount of change considerable, and in such cases the previous solutions should give results comparing favourably with those obtained by neglecting the change altogether, as is usual.

The same method will apply to any number of materials in contact, the surfaces of separation being coaxial cylinders. Thus, suppose there to be $i + 1$ materials whose elastic constants, proceeding outwards, are in order (m, n), (m_1, n_1), ... (m_i, n_i). Let the surfaces of separation be in order $r = a_1 \dots r = a_i$, the outmost surface of all being $r = a$, and the inmost, if the cylinder be hollow, $r = b$. Employing also suffixes to distinguish the constants for the several materials, we may take as the solution for the $(s+1)$th material, following (1),

$$\left.\begin{array}{l} \delta_s = 2\mathrm{A}_s + \mathrm{B}, \\ u_s = \mathrm{A}_s r + \dfrac{\mathrm{A}_s{'}}{r}, \\ w_s = \mathrm{B}z\,; \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (20)$$

where, as previously, the B is the same for all the media. At the common surface $r = a_s$ of this and the sth medium we have, precisely as in (9) and (10),

$$(m_{s-1} - n_{s-1})\mathrm{B} + 2m_{s-1}\mathrm{A}_{s-1} - \frac{2n_{s-1}}{a_s^2}\mathrm{A}'_{s-1} = (m_s - n_s)\mathrm{B}$$
$$+ 2m_s\mathrm{A}_s - \frac{2n_s}{a_s^2}\mathrm{A}_s{'}, \quad . \quad (21)$$

and

$$\mathrm{A}_{s-1}a_s + \frac{\mathrm{A}'_{s-1}}{a_s} = \mathrm{A}_s a_s + \frac{\mathrm{A}_s{'}}{a_s}. \quad . \quad . \quad . \quad (22)$$

At each of the i surfaces of separation there are two equations of the types (21) and (22). At the outmost surface $r=a$, neglecting atmospheric pressure as before, we have

$$(m_i - n_i)\mathrm{B} + 2m_i\mathrm{A}_i - \frac{2n_i}{a^2}\mathrm{A}'_i = 0 \; ; \quad . \quad . \quad (23)$$

and at the inmost surface $r=b$ a similar equation, writing b for a and dropping the suffix. If the cylinder be not hollow this last equation does not exist, but in its place we obviously have $\mathrm{A}'=0$. Thus there are $2i+2$ or $2i+1$ equations according as the cylinder is hollow or not, to determine the constants of types A and A$'$ in terms of B. Of these constants each medium possesses two, except when the cylinder is solid, when the central medium has only one constant, viz. A. Thus, whether the cylinder be hollow or not the equations are the same in number as the constants, which thus may all be directly expressed in terms of B. The values of the constants can be at once written down under the form of determinants. To determine B, we have

$$\mathrm{F} = \pi[(a_1^2 - b^2)\mathrm{R} + (a_2^2 - a_1^2)\mathrm{R}_1 + \ldots (a^2 - a_i^2)\mathrm{R}_i].$$

For the special case $\dfrac{m_s}{n_s} = $ constant for all values of s, of which we have previously seen the importance, a very simple solution holds. In fact it is obvious from (21), (22), and (23) that all our conditions are then satisfied by

$$\left. \begin{aligned} \mathrm{A}'_s &= \mathrm{A}'_{s-1} = \ldots = 0, \\ \mathrm{A}_s &= \mathrm{A}_{s-1} = \ldots = -\frac{m-n}{2m}\mathrm{B} = -\sigma\mathrm{B} \end{aligned} \right\} \; . \; (24)$$

in our previous notation, σ being, in accordance with this hypothesis, the same for all the media. In this case the equation for B is simply

$$\mathrm{F} = \pi\mathrm{B}[(a_1^2 - b^2)\mathrm{M} + (a_2^2 - a_1^2)\mathrm{M}_1 + \ldots + (a^2 - a_i^2)\mathrm{M}_i], \ldots (25)$$

where M, M_1, &c. denote Young's moduli for the several media. Also the solution (19) applies to all the media.

The equations (21) and (22) have been established independently of the absolute thickness of the layers : thus, under certain limitations to be presently considered, they may be supposed to hold when the thickness is indefinitely reduced, and thus in the limit to apply to a continuously varying medium. Thus, dropping the suffixes and writing r for a_{s+1}, we get in place of the constants of types A and A$'$ certain functions of

r determined by the equations

$$2\frac{d}{dr}(m\mathrm{A}) + \mathrm{B}\frac{d}{dr}(m-n) - \frac{2}{r^2}\frac{d}{dr}(n\mathrm{A}') = 0, \quad . \quad (26)$$

$$r^2\frac{d\mathrm{A}}{dr} + \frac{d\mathrm{A}'}{dr} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Being given the law of variation of m and n, we can from these equations get a differential equation in A or in A' as desired. To determine the constants of the solution, we have

$$2m\mathrm{A} + (m-n)\mathrm{B} - 2n\frac{\mathrm{A}'}{r^2} = 0 ;$$

when $r=a$, and also when $r=b$ if the cylinder be hollow ; if it be solid, instead of the latter equation we have $\mathrm{A}'=0$ when $r=0$. In obtaining these equations we have, it seems to me, tacitly assumed that the force at any point is the same as if all the neighbouring material, at least on one side of a plane through the point, within the distance at which molecular forces are sensible, were the same as at the point considered. Thus, if the variation in the material were very rapid, the validity of deductions from these equations might be questioned.

As an example of the use of (26) and (27), let us consider the case of a solid cylinder of which the material has elastic constants given by

$$\left.\begin{array}{l} m = m_0(1 + pr), \\ n = n_0\,(1 + qr); \end{array}\right\} \quad . \quad . \quad . \quad . \quad (28)$$

where m_0, n_0, p, q are absolute constants ; while $pa : 1$ and $qa : 1$ are so small that terms containing their squares or products may be neglected. The form of (26) and (27), then, suggests

$$\mathrm{A} = \mathrm{A}_0(1 + cr), \quad . \quad . \quad . \quad . \quad . \quad (29)$$

where A_0 and c are constants, the latter being of the same order of quantities as p and q. Then from (27) we get

$$\mathrm{A}' = -\mathrm{A}_0\frac{cr^3}{3}, \quad . \quad . \quad . \quad . \quad . \quad (30)$$

no constant being required as A' vanishes when $r=0$. Substituting (29) and (30) in (26) and retaining only the principal terms, we get

$$\mathrm{A}_0 c = -\frac{\mathrm{B}(m_0 p - n_0 q) + 2m_0 p\mathrm{A}_0}{2(m_0 + n_0)}. \quad . \quad . \quad (31)$$

The surface-condition $\mathrm{P}=0$ when $r=a$ gives, when the above

values of m, n, A, and A' are substituted in (3),

$$2A_0[m_5(1+pa)+ca(m_0+\tfrac{1}{3}n_0)]=-B[m_0-n_0$$
$$+(m_0p-n_0q)a]. \quad . \quad . \quad . \quad (32)$$

The first approximation gives

$$A_0=-\sigma_0B.$$

Writing this in (31), we get

$$A_0c=-\frac{Bn_0(p-q)}{2(m_0+n_0)}. \quad . \quad . \quad . \quad . \quad (33)$$

Substituting this value of c in (32), we get, after reduction, for a second approximation,

$$A_0=-B\left[\sigma_0+\frac{n_0^2(p-q)a}{3m_0(m_0+n_0)}\right]. \quad . \quad . \quad (34)$$

Introducing these values of A_0, c, A' in (1), we get, after reduction,

$$u=-\sigma_0Br\left[1+\frac{2}{3}\frac{n_0(p-q)}{m_0^2-n_0^2}(n_0a+m_0r)\right]; \quad . \quad . \quad (35)$$

while throughout,

$$w=Bz.$$

It should be noticed that correct values of P and R are to be obtained only from (3) and (4), substituting therein the above values of A, A', m, and n.

If, as previously, F denote the total traction over the terminal section, we get

$$F=\int_0^a 2\pi r R dr,$$

where R has the value (4), when m, n, and A are regarded as variables given by (28), (29), (33), and (34).

This equation easily leads to

$$F=\pi a^2 M_0 B\left[1+\frac{2}{3}aq+\frac{2}{3}a(p-q)\frac{n_0}{3m_0-n_0}\right], \quad . \quad (36)$$

where M_0 is the value of Young's modulus for the material at the axis.

When $\frac{m}{n}$ is constant, $p=q$; and the above value of B obviously agrees with that derived from (25), noticing that then $M=M_0(1+qr)$.

The torsion of a cylinder, hollow or solid, formed of different materials or of one continuously varying material, as in the cases just considered, presents no difficulty. Regarding

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first a succession of materials in contact, we see from (29a) and (30a) that, supposing the end $z=0$ fixed, a possible solution for any medium is

$$v = \mathrm{E}rz. \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

Further, this solution, regarding E as an absolute constant, will apply to all the media. For from (9a) we see that the only stress existing will be S, and at all the surfaces of separation, as well as at the bounding surfaces of the cylinder, $\mu = \nu = 0$; thus all the surface-equations (10a) are identically satisfied. Also the value of v is the same for any two adjacent media at their common surface. If G be the couple of torsion applied at the end of the cylinder, and if $a_1 \ldots a_i$, $n, n_1 \ldots n_i$ have their previous meaning, we get, to determine E,

$$\mathrm{G} = 2\pi\mathrm{E}\left[\int_b^{a_1} nr^3 dr + \int_{a_1}^{a_2} n_1 r^3 dr + \ldots + \int_{a_i}^{a} n\ r^3 dr\right], \quad . \quad (38)$$

i. e.

$$\mathrm{G} = \frac{\pi\mathrm{E}}{2}\left[n(a_1^4 - b^4) + n_1(a_2^4 - a_1^4) + \ldots + n_i(a^4 - a^4)\right]. \quad (39)$$

The values of a and b and the number of the media may be any whatever.

With limitations similar to the case of longitudinal traction this solution may be supposed to apply to a continuously varying medium, and the value of E will then be given by

$$\mathrm{G} = 2\pi\mathrm{E}\int_b^a nr^3 dr. \quad . \quad . \quad . \quad . \quad (40)$$

This last expression obviously includes (38), treating n as a discontinuous function.

The chief use of the preceding investigations would probably be in assigning the limit to the traction, pressure, or torsion which could be applied with safety to a structure of the kind considered. Unfortunately there seems no general agreement among practical men as to how the limits of safety may be fixed for a material when exposed to any system of force, except perhaps longitudinal traction. One theory that seems to meet with considerable approval is that, whatever the system of forces may be, the structure is safe so long as the greatest positive strain does not exceed a certain limit, to be determined experimentally for each separate material, presumably by uniform traction. As to the correctness of this limit for the case of uniform traction no doubt need be entertained. It does not follow that rupture will ensue as soon as this limit is passed ; but the nature of the material itself will be altered, and rupture will follow sooner or later.

In the case of traction the only positive strain is $\frac{dw}{dz}$ or B, which is the same for all the media of which the bar may consist. If, then, the traction F be increased till B exceed the limit of safety of any one material, that material will finally give way. Theoretically, of course, this material might give way uniformly all round, with the result that the traction F would then have to be supported by the remaining materials. This would lead to increased strain in all these ; but the structure as a whole would still be safe if this new strain were less than the limit of safety for each of the materials left. If the increased strain exceeded this limit then a second rupture would occur, and so on. In practice, owing to some want of symmetry in the distribution of the traction, or to slight inequality in the material, the first yielding material would probably crack and give way only in the neighbourhood of one point. This would alter the distribution of the traction, and might bring it to bear most largely on the strongest materials. If, however, the result were that the line of action of the resultant of the tractional forces got displaced to a finite distance from the axis of the cylinder, the strain would be considerably lessened at some points and considerably increased at others. It is obvious that such local increase of strain would be extremely dangerous. Thus the traction to be applied with safety to a composite bar of this kind should be calculated on the basis of the resultant strain not exceeding the limit of safety of the material for which the limit is least.

In the case of longitudinal pressure B is negative, and for a bar of one isotropic material the other two principal strains (viz. $\frac{du}{dr}$ and $\frac{u}{r}$) are each equal to $-\sigma$B. In accordance with the theory recently referred to, $-\sigma$B should not exceed the limit of safety as determined for the material by longitudinal traction. Others hold that the compression (*i. e.* B taken numerically) should not exceed a certain independent limit obtained from pressure experiments. The results we have obtained will enable the greatest pressure to be calculated which can be applied to a composite bar, without passing the limit of safety, for any one of the materials, determined in accordance with either of the above theories.

In the case of torsion the only existent strain is the shear $\frac{dv}{dz} = Er$, which is shown in any treatise on elastic solids to be equivalent to an equal extension and compression in directions making each an angle of 45° with the direction of shear, the

numerical measure of either being $\frac{1}{2}Er$. Thus, according to the theory first mentioned, in the case of torsion of a composite circular bar, if the couple of torsion be such that $\frac{1}{2}Er$ exceed at any distance from the axis the limit of safety, as determined by tractional experiments, for the material at that distance rupture will ensue. The rupture would at first be limited to the single material; and if it proceeded right round would merely produce an increased strain in the remaining media, which might or might not, according to circumstances, produce further rupture. In practice, rupture would in all probability be at first limited to a small region, and the bar would undoubtedly tend to become warped. Direct experiments on the rupture of isotropic bars by torsion may disprove the above theory, but it is fairly obvious that the true law must depend on the state of strain and stress in the material. Thus the preceding solution will in any case supply the data that may be necessary in determining the limit of safety of a composite bar under torsion.

XXXII. *Further Remarks on* Mr. Aitken's *Theory of Dew.* By CHARLES TOMLINSON, *F.R.S.**

I HAD no idea that the innocent title of my paper, "Remarks on a *New* Theory of Dew," had a guilty meaning; but according to Mr. Aitken I "was raising a false contention," and so attempting to place the author "in opposition to recognized authorities"; that the results of his investigation "are in no sense entitled to be called new;" and he repeatedly states that his investigation was not promulgated in opposition to the theory of Dr. Wells, but "in extension of the work, the foundations of which were laid by Dr. Wells." Again, he says the new theory "is not in opposition, nor are the results contrary to the teaching of Dr. Wells." Once more, the author "never made any attempt to set aside Wells's theory."

And yet it is curious to notice that 'Chambers's Journal' for May 29th last contains an article headed "A New Theory of Dew," in which the writer, after giving an accurate outline of Wells's theory, goes on to say that Mr. Aitken "has brought forward many observations, and the results of numerous experiments, which appear to prove that Dr. Wells' theory of dew is not, after all, correct." We are further informed that

* Communicated by the Author.

" the essential difference between the old and the new theories is as to the source of the moisture which forms the dew. Instead of being condensed from the air above by the cool vegetation, Mr. Aitken maintains that it comes from the ground."

Again the Chambers's article, referring to Wells, says :— " The points of the grass, small twigs, and all other good radiating surfaces are cooled the most; and accordingly we find the dew-drops most abundant on these bodies ; whilst on metal, or hard stone surfaces, which are poor radiators, we seldom or never find any dew." This is the Wells picture ; the writer now turns to the Aitken picture. " A closer observation reveals the fact that these so-called ' dewdrops ' are formed at the end of the minute veins of the leaves and grass, and are not now recognised as dew at all, but moisture exuded from the interior of the plants themselves."

And yet Mr. Aitken is angry with me for calling his theory *new*, and for asserting that, if true, it will supersede the labours of previous observers. He says :—" I do not find that he [Mr. Tomlinson] adduces any results of previous observers that are in any way rendered nugatory by the results set forth in my paper."

If Mr. Aitken would condescend to study the classical memoir of Melloni (an abstract of which occupies the greater part of my paper), I should be much surprised if he did not become a convert to its experimental methods and conclusions. But at present he sees through the spectacles of his own theory, and therefore cannot appreciate the force of Melloni's singular care with which he protected his thermometers from sharing in the radiation of the surrounding bodies, whose temperature they had to indicate ; for while other observers get differences of temperature between their two thermometers, amounting in some cases to as much as 16° or 18° F., Melloni is satisfied with a difference of only 2° or 3° C., and his theory of convection justifies this modest difference, and also accounts for many other phenomena, including the inverted trays and other objects which Mr. Aitken found wetted only on their under surfaces.

In like manner Mr. Aitken does not see the force of the observations made in Persia and the African Desert, seeing that his remarks " apply only to this climate." Surely the great forces of Nature rule as impartially in Persia and in Africa as in Scotland ; and where no aerial vapour exists, there is no deposit of dew. The cases given were intended to show that in the arid regions there was no dew ; but that long

before the travellers reached any considerable body of water, nocturnal dews were abundant, and they were deposited from the air, and did not rise out of the ground.

Mr. Aitken also remarks that my notice of the Florentine Academicians, of Robert Boyle, and Le Roi have no bearing on the subject. The bearing is that these early observers proved that the moisture which forms dew and hoar frost exists in the air, and does not exhale from the ground.

Mr. Aitken is also " puzzled to understand " what bearing Pictet's observation has on the subject. In the abstract in ' Nature ' of Mr. Aitken's memoir, it appears as an original discovery that " these observations made at night showed the ground at a short distance below the surface to be always hotter than the air over it." Pictet observed the same fact in 1779. So also in my account of the weighed turf, I certainly did not wilfully form a " misconception of the essential features of the experiment," when I compared it to objects which, when exposed on Patrick Wilson's scale-board, gained weight, while in Mr. Aitken's case the turf lost weight. It is true that my observations were founded on the abstract of the memoir contained in ' Nature.' In January last I wrote for a copy of the memoir, which was promised as soon as the ' Edinburgh Transactions ' were published. I waited until May and did not receive it. I inquired for it at the Royal Society in June, but it had not arrived, nor have I yet had the privilege of reading it. Mr. Aitken is therefore entitled to any advantage that may arise from my use of the abstract instead of the original memoir.

As I do not intend to write again on this subject, I conclude by assuring Mr. Aitken that I have no unfriendly feeling towards him ; but on the contrary freely admit that he has achieved much good scientific work, which I cannot but admire ; but as regards his new theory of Dew I think he has gone astray, and in the interests of scientific truth I have ventured to criticise it. The subject is one that has occupied a great deal of my attention, and there is no doubt in my mind that, if this theory be accepted, a large amount of excellent work on the part of first-rate observers must be set aside as false.

Highgate, N., August 9, 1886.

XXXIII. *On the Self-induction of Wires.*—Part II.
By Oliver Heaviside[*].

IN Part I. (p. 118) the inner conductor was solid. Let now the central portion be removed, making it a hollow tube of outer radius a_1 and inner a_0. The reason for this modification is that the theory of a tube is not the same when the return-conductor is outside as when it is inside it ; that is to say, it depends upon the position of the dielectric, the primary seat of the transfer of energy. The expression for H_1, the magnetic force at distance r from the axis, will now be

$$H_1 = \{J_1(s_1 r) - (J_1/K_1)(s_1 a_0)K_1(s_1 r)\} A_1 ; \quad . \quad . \quad (49)$$

instead of the former $A_1 J_1(s_1 r)$, of the first of equations (18); if we impose the condition $H_1 = 0$ at the inner boundary of the wire (as we may still call the inner tube). This means that there is to be no current from $r=0$ to $r=a_0$; we therefore ignore the minute longitudinal dielectric current in this space, just as we ignored that beyond $r=a_3$ previously. If we wish to necessitate that this shall be rigidly true, we may suppose that within $r=a_0$ and beyond $r=a_3$ we have not merely $k=0$, but also $c=0$, thus preventing current, either conducting or dielectric. In any case, with only $k=0$, the dielectric disturbance must be exceedingly small. On this point I may mention that my brother, Mr. A. W. Heaviside, experimenting with a wire and outer tube for the return, using a (for telegraphic purposes) very strong current, rapidly interrupted, and a sensitive telephone in circuit with a parallel outer wire, could not detect the least sign of any inductive action outside the tube, at least when the source of energy (the battery) was kept at a distance from the telephone. In explanation of the last remark, we need only consider that, although the transfer of energy is from the battery along the tubular space between the wire and return, yet, before getting to this confined space, there is a spreading out of the disturbances, so that in the neighbourhood of the battery the disk of a telephone may be strongly influenced by the variations of the magnetic field. On the other hand, the induction between parallel wires whose circuits are completed through the earth, is perceptible with the telephone at hundreds of miles distance, or practically at any distance, if the proper means be taken which theory points out. His direct experiments have, so far, only gone as far as forty miles, quite recently ; but this may easily be extended.

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[*] Communicated by the Author.

Corresponding to (49) we shall have

$$4\pi\Gamma_1 = s_1\{J_0(s_1 r) - (J_1/K_1)(s_1 a_0)K_0(s_1 r)\} A_1 ; \quad . \quad (50)$$

omitting, in both, the z and t factors. Now, to obtain the corresponding development of the general equation (22), we have only to change the $J_0(s_1 a_1)$ in it to the quantity in the $\{\}$ in (50) and the $J_1(s_1 a_1)$ to that in the $\{\}$ in (49), with $r = a_1$ in both cases.

The method by which (22) was got was the simplest possible, reducing to mere algebra the work that would otherwise involve much thinking out ; and, in particular, avoiding some extremely difficult reasoning relatively to potentials, scalar and vector, that would occur were they considered *ab initio*. But, having got (22), the interpretation is comparatively easy. Starting with the inner tube, (49) is the general solution of (14), with the limitation $H_1 = 0$ at $r = a_0$, if, in s, given by

$$-s_1^2 = 4\pi\mu_1 k_1 p + m^2,$$

we let p mean d/dt and m^2 mean $-d^2/dz^2$, instead of the constants in a normal system of subsidence, and let A_1 be an arbitrary function of z and t. Similarly, (50) gives us the connection between Γ and A_1. From it we may see what A_1 means. For, put $r = a_0$ in (50) ; then, since

$$(J_0 K_1 - J_1 K_0)(x) = -x^{-1},$$

we see that $A_1 = -4\pi a_0 K_1(s_1 a_0)\Gamma_0$, if Γ_0 is the current-density at $r = a_0$. When the tube is solid, $A_1 = 4\pi\Gamma_0/s_1$. But, without knowing A_1, (49) and (50) connect H_1 and Γ_1 directly, when A_1 is eliminated by division. Also $H_1 = C_1 \times (2/r)$, if C_1 be the total longitudinal current from $r = a_0$ to r ; hence

$$\Gamma_1 = \frac{s_1}{2\pi r} \frac{J_0(s_1 r) - (J_1/K_1)(s_1 a_0)K_0(s_1 r)}{J_1 \ldots - \ldots\ldots\ldots K_1 \ldots} C_1 \quad . \quad (51)$$

connects the current-density and the integral current.

Now pass to the outer tube. Quite similarly, remembering that $H_3 = 0$ at $r = a_3$, we shall arrive at

$$\Gamma_3 = \frac{s_3}{2\pi r} \frac{J_0(s_3 r) - (J_1/K_1)(s_3 a_3)K_0(s_3 r)}{J_1 \ldots - \ldots\ldots\ldots K_1 \ldots} C_3, \quad . \quad . \quad (52)$$

connecting Γ_3, the longitudinal current-density at distance r in the outer tube, with C_3, the current through the circle of radius r in the plane perpendicular to the axis.

Next, let there be longitudinal impressed electric forces in the wire and return, of uniform intensities e_1 and e_2, over the sections of the two conductors. We shall have

$$\rho_1\Gamma_1 = e_1 + E_1, \quad \rho_3\Gamma_3 = e_3 + E_3 ; \quad . \quad \bullet \quad . \quad . \quad (53)$$

if E_1 and E_3 are the longitudinal electric forces "of the field."
Therefore

$$e_1 - e_3 = e = \rho_1 \Gamma_1 - \rho_3 \Gamma_3 - (E_1 - E_3), \quad . \quad . \quad . \quad (54)$$

where e is the impressed force per unit length *in the circuit* at the place considered; the positive direction in the circuit being along the wire in the direction of increasing z, and oppositely in the return.

If in (51) we take $r = a_1$, and $r = a_2$ in (52), and use them in (54), then, since C_1 becomes C, the wire current, and C_3 becomes the same *plus* the longitudinal dielectric current, if we agree to ignore the latter, and can put $E_1 - E_3$ in terms of C, (54) will become an equation between e and C.

To obtain the required $E_1 - E_3$, consider a rectangular circuit in a plane through the axis, two of whose sides are of unit length parallel to z at distances a_1 and a_2 from the axis, and the other two sides parallel to r, and calculate the E.M.F. of the field in this circuit in the direction of the circular arrow. If z be positive from

left to right, the positive direction of the magnetic force through the circuit is upward through the paper. Therefore, if V be the line integral of the radial electric force from $r = a_1$ to $r = a_2$, so that dV/dz is the part of the E.M.F. in the rectangular circuit due to the radial force, we shall have

$$E_1 - E_3 + \frac{dV}{dz} = -\int_{a_1}^{a_2} \mu_2 \dot{H}_2 \, dr,$$

by the Faraday law, or equation (7); H_2 being the magnetic force in the dielectric. This being $2C/r$, on account of our neglect of Γ_2, we get, on performing the integration, $-L_0\dot{C}$, on the right side, where L_0 is the previously used inductance of the dielectric per unit length. This brings (54) to

$$e - \frac{dV}{dz} = L_0 pC + \frac{\rho_1 s_1}{2\pi a_1} \frac{J_0(s_1 a_1) - (J_1/K_1)(s_1 a_0) K_0(s_1 a_1)}{J_1 \ldots - \ldots \ldots K_1 \ldots} C$$

$$- \frac{\rho_3 s_3}{2\pi a_2} \frac{J_0(s_3 a_2) - (J_1/K_1)(s_3 a_3) K_0(s_3 a_2)}{J_1 \ldots - \ldots \ldots K_1 \ldots} C, (55)$$

which, for brevity, write thus,

$$e - \frac{dV}{dz} = L_0 pC + R_1''C + R_2''C, \quad . \quad . \quad . \quad (56)$$

where R_1'' and R_2'' define themselves in (55). They are generalized resistances of wire and return respectively, per unit length. But of their structure, later. Equation (56) is what

we get from (22) by treating $s_2 r$ as a small quantity and using (26); remembering also the extension from a solid to a hollow wire.

By more complex reasoning we may similarly put the right member of (54) in terms of C without the neglect of Γ_2, and arrive at (22) itself, in a form similar to (55) or (56). But we may get it from (22) at once by a proper arrangement of the terms, and introducing e. It becomes

$$e = \left(R_1'' + R_2'' \frac{R_{02}''}{R_{01}''} + R_{03}'' + \frac{R_1'' R_2''}{R_{01}''} \right) C. \quad . \quad (57)$$

Here R_1'' and R_2'' are as before, whilst R_{01}'' and R_{02}'' are similar expressions for the dielectric, on the assumption that $H = 0$ at $r = a_1$ or at $r = a_2$ respectively; thus,

$$R_{01}'' = + \frac{\rho_2 s_2}{2\pi a_2} \frac{J_0(s_2 a_2) - (J_1/K_1)(s_2 a_1) K_0(s_2 a_2)}{J_1 \ldots - \ldots \ldots \ldots K_1 \ldots},$$

$$R_{02}'' = - \frac{\rho_2 s_2}{2\pi a_1} \frac{J_0(s_2 a_1) - (J_1/K_1)(s_2 a_2) K_0(s_2 a_1)}{J_1 \ldots - \ldots \ldots \ldots K_1 \ldots}.$$

R_{03}'' has a different structure, being given by

$$R_{03}'' = - \frac{\rho_2 s_2}{2\pi a_1} \frac{J_0(s_2 a_1) - (J_0/K_0)(s_2 a_2) K_0(s_2 a_1)}{J_1 \ldots - \ldots \ldots \ldots K_1 \ldots}.$$

In these take $s_2 r$ small; they will become

$$R_{01}'' = R_{02}'' = \frac{\rho_2}{\pi(a_2^2 - a_1^2)};$$

that is, if ρ_2 be imagined to be resistivity, the steady flow resistance per unit length of the dielectric tube (fully, ρ_2 is the reciprocal of $k_2 + c_2 p/4\pi$); and, with $k_2 = 0$,

$$R_{03}'' = - \frac{s_2^2}{cp} 2 \log \frac{a_2}{a_1} = L_0 p + \frac{m^2}{Sp},$$

if S is the electrostatic capacity per unit length, such that $L_0 S = \mu_2 c_2$. Then (57) reduces to

$$e = (L_0 p + m^2/Sp + R_1'' + R_2'') C, \quad . \quad . \quad (58)$$

which is really the same as (56). For, by continuity, or by the second of (11),

$$- \frac{dC}{dz} = 2\pi a_1 \gamma_1 = 2\pi a_1 p\sigma = SpV, \quad . \quad . \quad (59)$$

if σ is the time-integral of the radial current at $r = a_1$, or, in other words, the electrification surface-density there, when the conductors are non-dielectric. (There is equal $-\sigma$ at the

$r = a_2$ surface). Therefore

$$-\frac{1}{\mathrm{S}p}\frac{d^2\mathrm{C}}{dz^2} = \frac{m^2}{\mathrm{S}p}\,\mathrm{C} = \frac{d\mathrm{V}}{dz}, \quad . \quad . \quad . \quad . \quad (60)$$

which establishes the equivalence.

Particular attention to the meaning of the quantity V is needed. It is the line-integral of the radial force in the dielectric from $r = a_1$ to $r = a_2$. Or it may be defined by

$$\mathrm{SV} = 2\pi a_1 \sigma = \mathrm{Q},$$

if Q be the charge per unit length of wire. But it is not the electric potential at the surface of the wire. It is not even the excess of the potential at the wire boundary over that at the inner boundary of the return. For, as it is the line-integral of the electric force from end to end of the tubes of displacement, it includes the line-integral of the electric force of inertia. It has, however, the obvious property of allowing us to express the electric energy in the dielectric in the form of a surface-integral, thus, $\frac{1}{2}\mathrm{V}\sigma$ per unit area of wire surface, or $\frac{1}{2}\mathrm{VQ}$ per unit length of wire, instead of by a volume integration throughout the dielectric. Hence the utility of V. The possibility of this property depends upon the comparative insignificance of the longitudinal current in the dielectric, which we ignore. It may happen, however, that the longitudinal displacement is far greater than the radial; but then it will be of so little moment that the problem could be taken to be a purely electromagnetic one. We need not use V at all, (58) being the equation between e and C without it. It is, however, useful in electrostatic problems, for the above-mentioned reason. Again, instead of V, we may use σ or Q, which are definitely localized.

The physical interpretation of the force $-d\mathrm{V}/dz$, in terms of Maxwell's inimitable dielectric theory, a theory which is spoiled by the least amount of tinkering, confusion and bemuddlement immediately arising, is sufficiently clear, especially when we assist ourselves by imagining the dielectric displacement to be a real displacement, elastically resisted, or any similar elastically resisted generalized displacement of a vector character. When there is current from the wire into the dielectric there is necessarily a back electric force in it due to the elastic displacement; and if it vary in amount along the wire, its variation constitutes a longitudinal electric force.

(58) being a differential equation previously, let in it m^2 be a constant. Then $\mathrm{R_1}''$ and $\mathrm{R_2}''$ may be thus expressed :—

$$\mathrm{R_1}'' = \mathrm{R_1}' + \mathrm{L_1}'p, \quad \mathrm{R_2}'' = \mathrm{R_2}' + \mathrm{L_2}'p, \quad . \quad . \quad . \quad (61)$$

where R_1' and R_2', L_1' and L_2' are functions of p^2. The utility of this notation arises from R_1' &c. becoming mere constants in simple-harmonically vibrating systems. Let e_m, V_m, and C_m be the corresponding quantities for the particular m; then, by (56),

$$e_m - \frac{dV_m}{dz} = L_0 p C_m + (R'_{1m} + L'_{1m} p) C_m + (R'_{2m} + L'_{2m} p) C_m. \quad (62)$$

Or

$$e_m - \frac{dV_m}{dz} = (R'_m + L'_m p) C_m, \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

where

$$R'_m = R'_{1m} + R'_{2m}; \quad L'_m = L_0 + L'_{1m} + L'_{2m}. \quad . \quad (64)$$

R'_m and L'_m are functions of p^2. Therefore, by (62), summing up,

$$e - \frac{dV}{dz} = \Sigma(R'_m + L'_m p) C_m. \quad . \quad . \quad . \quad (65)$$

Now, although R'_m and L'_m are really different functions of p^2 for every different value of m, since they contain m^2, yet if, in changing from one m to another, through a great many m's, from $m = 0$ upward, they should not materially change, we may regard R'_m and L'_m as having the $m = 0$ expressions, as in the purely electromagnetic case, and denote them by R' and L' simply. Then (65) becomes

$$e - \frac{dV}{dz} = (R' + L'p) C \quad . \quad . \quad . \quad . \quad . \quad (66)$$

simply. The equation of V is now

$$-\frac{de}{dz} + \frac{d^2V}{dz^2} = (R' + L'p) S p V; \quad . \quad . \quad . \quad (67)$$

and that of C_m being

$$e_m = (R'_m + L'_m p + m^2/S p) C_m \quad . \quad . \quad . \quad (68)$$

in the m case, that of C becomes now simply

$$S p e + \frac{d^2C}{dz^2} = (R' + L'p) S p C. \quad . \quad . \quad . \quad (69)$$

The assumption above made is, in general, justifiable.

Let us now compare these equations with the principal ways that have been previously employed to express the conditions of propagation of signals along wires. For simplicity, leave out the impressed force e. First, we have Ohm's system, which may be thus written:—

$$-\frac{dV}{dz} = R C, \quad -\frac{dC}{dz} = S p V, \quad \frac{d^2V}{dz^2} = R S p V. \quad . \quad (70)$$

Here the first equation expresses Ohm's law. C is the wire

current, R the resistance per unit length, and V is a quantity whose meaning is rather indistinct in Ohm's memoir, but which would be now called the potential. The second equation is of continuity. Misled by an entirely erroneous analogy, Ohm supposed electricity could accumulate in the wire in a manner expressed by the second of (70), wherein S therefore depends upon a specific quality *of the conductor*. The third equation results from the two previous, and shows that V, or C, or Q = SV diffuse themselves through the wire as heat does by differences of temperature when there is no surface loss. This system has at present only historical interest. The most remarkable thing about it is the getting of equations correct in form, at least approximately, by entirely erroneous reasoning.

The matter was not set straight till a generation later, when Sir W. Thomson arrived at a system which is formally the same as (70), but in which V is precisely defined, whilst S changes its meaning entirely. V is now to be the electrostatic potential, and S is the electrostatic capacity *of the condenser* formed by the opposed surfaces of the wire and return with dielectric between. The continuity of the current in the wire is asserted; but it can be discontinuous at its surface, where electricity accumulates and charges the condenser. In short, we simply unite Ohm's law (with continuity of current in the conductor) and the similar condenser law. The return is supposed to be of no resistance, and V = 0 at its boundary.

The next obvious step is to bring the electric force of inertia into the Ohm's law equation, and make the corresponding change in that of V ; that is, if we decide to accept the law of quasi-incompressibility of electricity in the conductor, which is implied by the second of (70), when Sir W. Thomson's meanings of S and V are accepted. Kirchhoff seems to have been the first to take inertia into account, arriving at an equation of the form $d^2V/dz^2 = (R + Lp)SpV.$

I am, unfortunately, not acquainted with his views regarding the continuity of the current, so that, translated into physical ideas, his equation may not be conformable to Maxwell's ideas, even as regards the conductor. Also, as his estimation of the quantity L was founded upon Weber's hypothesis, it may possibly turn out to be different in value from that in the next following system. In ignorance of Kirchhoff's investigation, I made the necessary change of bringing in the electric force of inertia in a paper " On the Extra Current " (Phil. Mag. August 1876), getting this system,

$$-\frac{dV}{dz} = (R + Lp)C, \quad -\frac{dC}{dz} = SpV, \quad \frac{d^2V}{dz^2} = (R + Lp)SpV, \quad (71)$$

wherein everything is the same as in Sir W. Thomson's system, with the addition of the electric force of inertia $-LpC$, where L is the coefficient of self-induction, or, as I now prefer to call it, for brevity, the inductance, per unit length of the wire, according to Maxwell's system, being numerically equal to twice the energy, per unit length of wire, of the unit current in the wire, uniformly distributed. Coming after Maxwell's treatise, there is of course no question of any important step in advance here, except perhaps in the clearing away of hypotheses involved in Kirchhoff's investigation.

The system (71) is amply sufficient for all ordinary purposes, with exceptions to be later mentioned. It applies to short lines as well as to long ones ; whereas the omission of L, reducing (71) to (70), renders the system quite inapplicable to lines of moderate length, as the influence of S tends to diminish as the line is shortened, relatively to that of L. An easily made extension of (71) is to regard R as the sum of the steady-flow resistances of wire and return, and V as the quantity Q/S, Q being the charge per unit length of wire. Nor are we, in this approximate system (71), obliged to have the return equidistant from the wire. It may, for instance, be the earth, or a parallel wire, with the corresponding changes in the formulæ for the electrostatic capacity and inductance.

But there are extreme cases when (71) is not sufficient. For example, an iron wire, unless very fine, by reason of its high inductivity ; a very thick copper wire, by reason of thickness and high conductivity ; or, a very close return current, in which case, no matter how fine a wire may be, there is extreme departure from uniformity of current distribution in the variable period ; or, extremely rapid reversals of current, for, no matter what the conductors may be, by sufficiently increasing the frequency we approximate to surface conduction.

We must then, in the system (71), with the extension of meaning of R and V just mentioned, change R and L to R′ and L′, as in (67), and other equations. In a S.H. problem, this simply changes R and L from certain constants to others, depending on the frequency. But, in general, it would I imagine be of no use developing R_1'' &c. in powers of p, so that we must regard $(R_1' + L_1'p)$ &c. merely as a convenient abbreviation for the R_1'' &c. defined by (56) and (55).

A further refinement is to recognise the differences between R′ and L′ in one m system and another, instead of assuming $m = 0$ in R_m''. And lastly, to obtain a complete development, and exact solutions of Maxwell's equations, so as to be able to fully trace the transfer of energy from source to sink, fall

back upon (57), or (22), and the normal systems (18) of Part I.

Now, as regards our obtaining the expansions of R_1' &c. in powers of p^2, we have to expand the numerators and the denominators of R_1'' and R_2'' in powers of p, perform the divisions, and then separate into odd and even powers. When the wire is solid, the division is merely of $\frac{1}{2}x J_0(x)$ by $J_1(x)$, a comparatively easy matter. The solid wire R' and L' expansions were given by Lord Rayleigh (Phil. Mag. May 1886). I should mention that my abbreviated notation was suggested by him. But in the tubular case, the work is very heavy, so, on account of possible mistakes, I go only as far as p^2, or three terms in the quotient. The work does not need to be done separately for the inner and the outer tube, as a simple change converts one R' or L' into the other. Thus, in the case of the inner tube, we shall have

$$R_1' = R_1 \left[1 + n^2(\mu_1 k_1 \pi a_1^2)^2 \left\{ \tfrac{1}{12} - \tfrac{2}{3}\frac{a_0^2}{a_1^2} + \tfrac{7}{12}\frac{a_0^4}{a_1^4} - \frac{2a_0^4 \log(a_1/a_0)}{a_1^2(a_1^2-a_0^2)} \right. \right. $$
$$\left. \left. + \frac{4a_0^6\{\log(a_1/a_0)\}^2}{a_1^2(a_1^2-a_0^2)^2} \right\} \right], \quad (72)$$

$$L_1' = R_1(\mu_1 k_1 \pi a_1^2) \left\{ \tfrac{1}{2} - \tfrac{3}{2}\frac{a_0^2}{a_1^2} + 2\log\frac{a_1}{a_0} \cdot \frac{a_0^4}{a_1^2(a_1^2-a_0^2)} \right\}, \quad (73)$$

where n^2 is written for $-p^2$, for the S.H. application.

As for L_1', it is simply the inductance of the tube per unit length (of the tube only), as may be at once verified by the square of force method. The first correction depends upon p^3. But R_1' gives us the first correction to R_1, which is the steady-flow resistance, so it is of some use. To obtain R_2' and L_2' from these, change R_1 to R_2, μ_1 and k_1 to μ_3 and k_3, a_0 to a_3, and a_1 to a_2. Or, more simply, (72) and (73) being the tube-formulæ when the return is outside it, if we simply exchange a_0 and a_1 we shall get the formulæ for the *same* tube when the return is inside it.

If the tube is thin, there is little change made by thus shifting the locality of the return. But if a_1/a_0 be large, there is a large change. This will be readily understood by considering the case of a wire whose return is outside it, and of great bulk. Although the steady resistance of the return may be very low, yet the percentage correction will be very large, compared with that for the wire.

Taking $a_1/a_0 = 2$ only, we shall find

$$R_1' = R_1[1 + (\pi k_1 a_1^2 \mu_1 n)^2 \times \cdot 012]$$

when the return is outside, and

$$R_1' = R_1[1 + (\pi k_1 a_0^2 \mu_1 n)^2 \times \cdot503]$$
$$= R_1[1 + (\pi k_1 a_1^2 \mu_1 n)^2 \times \cdot031]$$

when the return is inside. In the case of a solid wire, the decimals are $\cdot083$, so that whilst the correction is reduced, in this $a_1/a_0 = 2$ example, the reduction is far greater when the return is outside than when it is inside.

The high-speed tube formulæ are readily obtained. Those for the inner tube are the same as for a solid wire, and that for the outer tube depends not on its bulk, but on its inner radius. That is, in both cases it is the extent of surface that is in question, next the dielectric, from which the current is transmitted into the conductors. Let $G_0(x) = (2/\pi) K_0(x)$, and $G_1(x) = (2/\pi) K_1(x)$; then, when x is very large,

$$\left. \begin{array}{l} J_0(x) = -G_1(x) = (\sin x + \cos x) \div (\pi x)^{\frac{1}{2}} \\ J_1(x) = G_0(x) = (\sin x - \cos x) \div (\pi x)^{\frac{1}{2}} \end{array} \right\} \quad . \quad (74)$$

Use these in the R_1'' fraction, and put in the exponential form. We shall obtain

$$R_1'' = (\rho_1 s_1 i)/(2\pi a_1).$$

But

$$\tfrac{1}{2} s_1 a_1 i = (\pi k_1 \mu_1 p)^{\frac{1}{2}} a_1,$$

therefore

$$R_1'' = (\mu_1 \rho_1 p/\pi a_1^2)^{\frac{1}{2}}.$$

But

$$p^2 = -n^2,$$

therefore

$$p^{\frac{1}{2}} = (\tfrac{1}{2}n)^{\frac{1}{2}}(1+i) = (\tfrac{1}{2}n)^{\frac{1}{2}} + p(\tfrac{1}{2}n^{-1})^{\frac{1}{2}},$$

so that, finally,

$$R_1' = \frac{(\mu_1 \rho_1 q)^{\frac{1}{2}}}{a_1}, \quad L_1' = \frac{R_1'}{n}, \quad . \quad . \quad . \quad (75)$$

$q = n/2\pi =$ the frequency. To get R_2' and L_2', change the μ and ρ of course, and also a_1 to a_2.

It is clear that the thinner the tube, the greater must be the frequency before these formulæ can be applicable. For the steady-flow resistance is increased indefinitely by reducing the thickness of the tube, whilst the high-speed resistance is independent of the steady-flow resistance, and must be much greater than it. In (75) then, q must be great enough to make R' several times R, itself very large when the tube is very thin. Consequently thin tubes, as is otherwise clear,

may be treated as linear conductors, subject to the equations (71), with no corrections, except under extreme circumstances. The L may be taken as L_0, except in the case of iron.

I will now give the S.H. solution in the general case, subject to (58). Let there be any distribution of e (longitudinal, and of uniform intensity over cross sections). Expand it in the Fourier series appropriate to the terminal conditions at $z = 0$ and l. For definiteness, let wire and return be joined direct, without any terminal resistances. Then, $e_0 \sin nt$ being e at distance z, the proper expansion is

$$e_0 = e_{00} + e_{01} \cos m_1 z + e_{02} \cos m_2 z + \dots,$$

where $m_1 = \pi/l$, $m_2 = 2\pi/l$, &c. [It should be remembered that e is the $e_1 - e_2$ of (54) and (53). Shifting impressed force from the wire to the return, with a simultaneous reversal of its direction, makes no difference in e. Thus two e's directed the same way in space, of equal amounts, and in the same plane $z = $ constant, one in the inner, the other in the outer conductor, cancel. This will clearly become departed from as the distance of the return from the wire is increased.] Then, in the equation

$$e_m = (R'_m + L'_m p)C_m + (m^2/Sp)C_m$$
$$= R'_m C_m + (L'_m - m^2/Sn^2)pC_m,$$

we know e_m; whilst R'_m and L'_m are constants. The complete solution is obtained by adding together the separate solutions for e_{00}, e_{01}, &c., and is

$$C = \frac{1}{l} \left\{ \frac{e_{00} \sin(nt - \theta_0)}{(R'^2 + L'^2 n^2)^{\frac{1}{2}}} + 2\Sigma \frac{e_{0m} \sin(nt - \theta_m) \cos mz}{[R'^2_m + (L'_m - m^2/Sn^2)^2 n^2]^{\frac{1}{2}}} \right\}, (76)$$

where the summation includes all the m's, and

$$\tan \theta_m = (L'_m - m^2/Sn^2)n \div R'_m.$$

A practical case is, no impressed force anywhere except at $z = 0$, one end of the line, where it is $V_0 \sin nt$. Then, imagining it to be V_0/z_1 from $z = 0$ to $z = z_1$, and zero elsewhere, and diminishing z_1 indefinitely, the expansion required is

$$V_0/z_1 = (V_0/l)(1 + 2\Sigma \cos j\pi z/l),$$

j going from $1, 2, \dots$ to ∞. This makes the current solution become

$$C = \frac{V_0}{l} \left\{ \frac{\sin(nt - \theta_0)}{(R'^2 + L'^2 n^2)^{\frac{1}{2}}} + 2\Sigma \frac{\sin(nt - \theta_m) \cos mz}{\{R'^2_m + (L'_m - m^2/Sn^2)^2 n^2\}^{\frac{1}{2}}} \right\}. (77)$$

If the line is short, neglect the summation altogether, unless

U 2

the speed is excessive. Now (77) may perhaps be put in a finite form when R'_m is allowed to be different from R', though I do not see how to do it. But when $R'_m = R'$ and $L'_m = L'$ it can of course be done, for we may then use the finite solutions of (66) and (67). Thus, given $V = V_0 \sin nt$ at $z = 0$, and no impressed force elsewhere, find V and C everywhere subject to (66) and (67) with $e = 0$, and $V = 0$ at $z = l$.

Let

$$P = (\tfrac{1}{2} Sn)^{\frac{1}{2}} \{(R'^2 + L'^2 n^2)^{\frac{1}{2}} - L'n\}^{\frac{1}{2}}, \\ Q = \dots \{(\dots \dots)^{\frac{1}{2}} + \dots \}^{\frac{1}{2}}, \Bigg\} \quad . \quad (78)$$

$$\tan \theta_2 = \sin 2Ql \div (\epsilon^{-2Pl} - \cos 2Ql), \\ \tan \theta_1 = (L'nP - R'Q) \div (R'P + L'nQ); \Bigg\} \quad . \quad (79)$$

then the finite V and C solutions are

$$V = V_0 \epsilon^{-Pz} \sin (nt - Qz)$$

$$+ V_0 \frac{\epsilon^{Pz} \sin (nt + Qz + \theta_2) - \epsilon^{-Pz} \sin (nt - Qz - \theta_2)}{\epsilon^{Pl}(\epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql)^{\frac{1}{2}}}, \quad (80)$$

$$C = V_0 \frac{(Sn)^{\frac{1}{2}}}{(R'^2 + L'^2 n^2)^{\frac{1}{4}}} \Bigg[\epsilon^{-Pz} \sin (nt - Qz - \theta_1)$$

$$- \frac{\epsilon^{Pz} \sin (nt + Qz - \theta_1 + \theta_2) + \epsilon^{-Pz} \sin (nt - Qz - \theta_1 + \theta_2)}{\epsilon^{Pl}(\epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql)^{\frac{1}{2}}} \Bigg]. \quad (81)$$

If we expand this last in cosines of mz we shall obtain (77), with $R'_m = R'$. There are three waves; the first is what would represent the solution if the line were of infinite length; being of finite length there is a reflected wave (the ϵ^{Pz} term), and another reflected at $z = 0$, the third and least important.

The amplitude of C anywhere is

$$V_0 \frac{(Sn)^{\frac{1}{2}}}{(R'^2 + L'^2 n^2)^{\frac{1}{4}}} \Bigg[\frac{\epsilon^{2P(l-z)} + \epsilon^{-2P(l-z)} + 2 \cos 2Q(l-z)}{\epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql} \Bigg]^{\frac{1}{2}}.$$

At the distant $z = l$ end it is

$$C_0 = 2V_0 \frac{(Sn)^{\frac{1}{2}}}{(R'^2 + L'^2 n^2)^{\frac{1}{4}}} (\epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql)^{-\frac{1}{2}}. \quad (82)$$

I have already spoken of the apparent resistance of a line as its impedance (from impede). The steady flow impedance is the resistance. The short line impedance is $(R^2 + L^2 n^2)^{\frac{1}{2}} l$

or $(R'^2 + L'^2 n^2)^{\frac{1}{2}} l$, at the frequency $n/2\pi$, according as current density differences are, or are not, ignorable. The impedance according to the latter formula increases with the speed, but is greater or less than that of the former formula (linear theory) according as the speed is below or above a certain speed.

But if the speed is sufficiently increased, even on a short line, the formula ceases to represent the impedance, whilst, if the line be long it will not do so at any speed except zero. According to (82) we have

$$V_0/C_0 = \frac{(R'^2 + L'^2 n^2)^{\frac{1}{4}}}{2(Sn)^{\frac{1}{2}}} \left(\epsilon^{2Pl} + \epsilon^{-2Pl} - 2\cos 2Ql\right)^{\frac{1}{2}}, \quad (83)$$

as the distant end impedance of the line. That is, we have extended the meaning of impedance, as we must (or else have a new word), since the current-amplitude varies as we pass from beginning to end of the line. (83) will, roughly speaking, on the average, give the greatest value of the impedance. It is what the resistance of the line would have to be in order that when an S.H. impressed force acts at one end, the current-amplitude at the distant end should be, without any electromagnetic and electrostatic induction, what it really is. The distant end impedance may easily be less than the impedance according to the electromagnetic reckoning. What is more remarkable, however, is that it may be much less than the steady-flow resistance of the line. This is due to the to-and-fro reflection of the dielectric waves, which is a phenomenon similar to resonance.

To show this, take $R' = 0$ in the first place, which requires the conductors to be of infinite conductivity. Then $L' = L_0$, the dielectric inductance. We shall have, by (83) and (78),

$$V_0/C_0 = L_0 v \sin(nl/v), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (84)$$

where $v = (L_0 S)^{-\frac{1}{2}} = (\mu_2 c_2)^{-\frac{1}{2}}$, the speed of waves through the dielectric when undissipated. The sine is to be taken positive always. If $nl/v = \pi$, 2π, &c., the impedance is zero, and the current-amplitude infinite. Here $nl/v = \pi$ means that the period of a wave equals the time taken to travel to the distant end and back again, which accounts for the infinite accumulation, which is, of course, quite unrealizable.

Now, giving resistance to the line, it is clear that although the impedance can never vanish, it will be subject to maxima and minima values as the speed increases continuously, itself increasing, on the whole. We may transform (83) to

$$V_0/C_0 = (R'^2 + L'^2 n^2)^{\frac{1}{2}} l \left[\left(\frac{v'}{nl} \right)^2 \sin^2 \left(\frac{nl}{v'} \right) + \left(\frac{nl}{v'} \right)^4 \frac{h}{90} \left\{ 1 - \frac{1}{7} \left(\frac{nl}{v'} \right)^2 \right. \right.$$

$$+ \frac{1}{105} \left(\frac{nl}{v'} \right)^4 (1 + \frac{1}{12}h) - \frac{4}{105.99} \left(\frac{nl}{v'} \right)^6 (1 + \frac{3}{16}h)$$

$$+ \frac{10}{105.99.91} \left(\frac{nl}{v'} \right)^8 (1 + \frac{3}{10}h + \frac{1}{80}h^2) - \dots \right\} \Big]^{\frac{1}{2}}, \quad (85)$$

where

$$v' = (L'S)^{-\frac{1}{2}}, \text{ and } h = (R'/L'n)^2.$$

The factor outside the [ ] is the electromagnetic impedance ; and, if we take only the first term within the [ ], we shall obtain the former infinite conductivity formula (84). The effect of resistance is shown by the terms containing h.

With this v' and h notation (83) becomes

$$V_0/C_0 = \frac{1}{2} L'v' (1+h)^{\frac{1}{4}} \{ \epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql \}^{\frac{1}{2}} ; \quad (86)$$

where

$$Ql = (nl/v')(\sqrt{1+h} + 1)^{\frac{1}{2}} \div \sqrt{2},$$

$$Pl = (nl/v')(\sqrt{1+h} - 1)^{\frac{1}{2}} \div \sqrt{2}.$$

Choose Q so that $2Ql = 2\pi$, and let $h = 1$. This requires $nl/v' = 2.85$. Then

$$V_0/C_0 = \frac{1}{2} L'v' \cdot 2^{\frac{1}{4}} \left[\epsilon^{.8284\pi} + \epsilon^{-\dots} - 2 \right]^{\frac{1}{2}},$$
$$= 60.6 \text{ L' ohms,}$$

if we take $v = 30^{10}$ cm. $= 30$ ohms. This implies $L' = L_0$, and the dielectric air. Without making use of current-density differences, we may suppose that the conductors are thin tubes. Therefore,

$$\frac{\text{Impedance}}{\text{Resistance}} = \frac{60.6 \text{ L'} \cdot 10^9}{R'l} = \text{about } \frac{202}{285},$$

by making use of the above values of h and nl/v'.

But take $2Ql = \frac{1}{2}\pi$, or one fourth of the above value. Then

$$V_0/C_0 = 28 \text{ L' ohms,}$$

and

$$\frac{\text{Impedance}}{\text{Resistance}} = \text{about } \frac{4}{3}.$$

Thus the amplitude of the current, from being less than the steady-flow strength in the last case, becomes 42 per cent. greater than the steady-flow current by quadrupling nl/v', and keeping $h = 1$. We have evidently ranged from somewhere near the first maximum to the first minimum value of the impedance. These figures suit lines of any length, if we

choose the resistances &c. properly. The following will show how the above apply practically. Remember that 1 ohm per kilom. $= 10^4$ per cm. Then, if $l_1 =$ length of line in kilom.,

If $R' = 10^3$, and $L' = 1$, $\therefore$ $n = 10^3$, and $l_1 = 856$,

,, $R' = 10^3$, ,, $L' = 10$, ,, $n = 10^2$, ,, $l_1 = 8568$,

,, $R = 10^4$, ,, $L' = 1$, ,, $n = 10^4$, ,, $l_1 = 85$,

,, $R = 10^4$, ,, $L' = 10$, ,, $n = 10^3$, ,, $l_1 = 856$,

,, $R = 10^4$, ,, $L' = 100$, ,, $n = 10^2$, ,, $l_1 = 8568$,

,, $R' = 10^5$, ,, $L' = 1$, ,, $n = 10^5$, ,, $l_1 = 8\cdot5$,

,, $R' = 10^5$, ,, $L' = 10$, ,, $n = 10^4$, ,, $l_1 = 85$,

,, $R' = 10^5$, ,, $L' = 100$, ,, $n = 10^3$, ,, $l_1 = 856$,

,, $R' = 10^6$, ,, $L' = 10$, ,, $n = 10^5$, ,, $l_1 = 8\cdot5$.

The resistances vary from $\frac{1}{10}$ to 100 ohms per kilom., the inductances from 1 to 100 per cm., the frequencies from $10^2/2\pi$ to $10^5/2\pi$, and the lengths from $8\cdot5$ to 8568 kilom. In all cases $\frac{2}{3}$ is the ratio of the distant end impedance to the resistance. The common value of nl_1 is 856800.

In the other case, nl/v' has one fourth of the value just used, so that, with the same R' and L', l_1 has values one fourth of those in the above series.

Telephonic currents are so rapidly undulatory (it is the upper tones that go to make good articulation, and convert mumblings and murmurs into something like human speech) that it is evident there must be a considerable amount of this dielectric resonance, if a tone last through the time of several wave periods.

Having got the solution for C, the wire current, we may obtain those for H, Γ, and γ from it. Thus, H_r being the same as $(2/r)C_r$, where C_r is the longitudinal current through the circle of radius r, we may first derive C_r or H_r from C, and then derive Γ and γ from either by (11). Thus, make use of (49) and (50), and the value of A_1 there given. Then we shall obtain

$$C_r = \frac{r}{a_1} \frac{J_1(s_1 r) - (J_1/K_1)(s_1 a_0) K_1(s_1 r)}{J_1(s_1 a_1) - (J_1/K_1)(s_1 a_0) K_1(s_1 a_1)} C, \quad . \quad . \quad (87)$$

where, in the s_1, p and m^2 are to be d/dt and $-d^2/dz^2$. Similarly for the return tube.

In a comprehensive investigation, the C solution would be only a special result ; as this special result is more easily got by itself, it might appear that there would be some saving of labour by first getting the C solution and then deriving from

it the general. But this does not stand examination; the work has to be done, whether we derive the special results from the general, or conversely.

In the solid wire case

$$C_r = \frac{r}{a_1} \frac{J_1(s_1 r)}{J_1(s_1 a_1)} C,$$

or

$$C_r = \frac{r^2}{a_1^2} \left\{ 1 + \tfrac{1}{2}(\pi\mu_1 k_1 p + \tfrac{1}{4} m^2)(r^2 - a_1^2) \right.$$

$$+ \tfrac{1}{3} \frac{1}{1^2 2^2} (\pi\mu_1 k_1 p + \tfrac{1}{4} m^2)^2 (r^2 - a_1^2)(r^2 - 2a_1^2)$$

$$\left. + \tfrac{1}{4} \frac{1}{1^2 2^2 3^2} (\pi\mu_1 k_1 p + \tfrac{1}{4} m^2)^3 (r^2 - a_1^2)(r^4 - 5r^2 a_1^2 + 7a_1^4) \right\} + \ldots \Bigg\} C.$$

Or, use the M and N functions of Part I., equations (42). For we have

$$J_0(s_1 r) \doteq (M + iN)(s_1 r i^{\frac{1}{2}}),$$

where $s_1 r i^{\frac{1}{2}}$ takes the place of the y in those equations. M contains the even and N the odd powers of $(p + m^2/4\pi\mu_1 k_1)$.

We have also

$$\Gamma_r = J_0(s_1 r)\Gamma_0 = \frac{s_1}{2\pi a_1} \frac{J_0(s_1 r)}{J_1(s_1 a_1)} C,$$

Γ_0 being Γ at $r = 0$; and, since by the first of these

$$\Gamma_{a_1} = J_0(s_1 a_1)\Gamma_0$$

connects the boundary and axial current-densities, we see that the ratio of their amplitudes in the S.H. case is

$$(M^2 + N^2)^{\frac{1}{2}},$$

using the $r = a_1$ expressions, with $m = 0$.

I hope to be able to conclude this paper in a third part.

XXXIV. *Further Notes on the Formulæ of the Electromagnet and the Equations of the Dynamo.* By Professor SILVANUS P. THOMPSON, *D.Sc., B.A.*[*]

1. *The Lamont-Frölich Formula.*

DR. O. FRÖLICH has done me the honour of replying[†] to a certain point in my former communication to the Physical Society, "On the Law of the Electromagnet and the Law of the Dynamo"[‡]. In that communication I pointed

out that Lamont had in 1867 published a rational theory of the electromagnet, based upon the assumption that the permeability of the iron was at every stage of the magnetization proportional to the deficit of saturation, leading him to an exponential expression,

$$m = \mathrm{M}(1 - \epsilon^{-kx}),$$

where m is the magnetism present at any stage, M its maximum value, k the ratio of the permeability to the deficit of saturation, and x the magnetizing force proportional (approximately) to the number of ampere-turns of the magnetizing current. This formula more correctly expressed the facts than either of the commoner formulæ of Lenz and Jacobi and of Müller.

I further pointed out that Lamont had himself* given, as a sufficient approximation to the formula, the simpler expression,

$$m = \frac{a\mathrm{M}x}{\mathrm{M} + ax},$$

which formula is mathematically identical with that now commonly attributed to Dr. Frölich. For, writing $a = k\mathrm{M}$, we get at once

$$m = \mathrm{M}\frac{kx}{1 + kx},$$

which is the formula claimed by Frölich.

Lamont having developed his exponential expression in a series of ascending powers of kx, I did the same for the simpler formula for the purpose of comparison, and showed that, *neglecting the fourth and higher terms* of each series, the expansions are very nearly equal for all values of kx except for very large ones, and are identical for the value $kx = \frac{3}{5}$. Dr. Frölich, overlooking the words I have above italicized, commits the mistake of supposing that I had said that Lamont's exponential expression is identical in value with the simpler formula when $kx = \frac{3}{5}$. I have said nothing of the kind.

Further, when Dr. Frölich says, " Hiernach ist die Aussicht vorhanden dass nicht die Lamontsche sondern die von mir benutzte Formel die wahre Gesetz der Elektromagnete enthält," he is forgetting that the formula used by him is also Lamont's. He has proved, in his most recent communication, that the differences between the calculated and the observed values are about half as great when calculated by the simpler formula. The second and simpler formula suggested by Lamont appears therefore to be better than the first and more

* Lamont, *Magnetismus*, p. 41.

complex formula which he suggested. It has been recently shown by Mr. Bosanquet* that if we take as expressive of the permeability, not the instantaneous value dm/dx, but the integral value m/x, and treat this on Lamont's plan as proportional to the deficit of saturation,

$$\frac{m}{x} = k(\mathrm{M}-m),$$

we deduce at once the formula in question. Dr. Frölich's researches upon the dynamo have given us the most complete and perfect proofs of the adequacy of the formula to represent the facts of the electromagnet as it is used in practice. My former communication was indeed mainly written to point out the extreme value and interest of Dr. Frölich's work from this point of view.

2. *Frölich's simplified Formula of the Electromagnet.*

Since my former communication to the Physical Society was made, a further work on the theory of the dynamo by Dr. Frölich has appeared†. In this work he carries the simplification of the formulæ of magnet and of dynamo one stage further by introducing considerations somewhat closely connected with those which entered into my own work of 1883–4.

The form adopted by me in 1883 for the Lamont-Frölich formula was

$$\mathrm{H} = \frac{\mathrm{G}\kappa\mathrm{S}i}{1+\sigma\mathrm{S}i};$$

where H is the resulting average intensity of the magnetic field (in which the armature rotates), κ the initial value of the magnetic permeability, G a coefficient depending upon such purely geometrical quantities as the form and size of core, pole-pieces, and coils, S the number of windings of the magnetizing coil, and σ the "saturation-coefficient." This can be transformed at once to Frölich's form by writing $\mathrm{M}=\mathrm{G}\kappa/\sigma$; $k=\sigma$; $x=\mathrm{S}i$. In 1884 I pointed out‡ the nature of this saturation-constant and its importance in the resulting equations of the dynamo. It is the reciprocal of that number of ampere-turns which, to mark its importance, I ventured to term "diacritical," namely *that number of ampere-turns which will reduce the instantaneous value of the magnetic permeability to half its initial value,* or which, in the formula used, will give

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* 'Electrician,' vol. xvi. p. 247, February 1886.

† *Die dynamoelektrische Maschine. Eine physikalische Beschreibung für den technischen Gebrauch,* von Dr. O. Frölich (Berlin, 1886).

‡ 'Dynamoelectric Machinery,' first edition, p. 221; also Report Brit. Assoc., Montreal Meeting, 1884.

to the magnet exactly *half its maximum magnetism.* I further pointed out in my lectures on the dynamo that year, that, if the number of windings of the coil S is given, there will be a " diacritical " current, namely a particular value of current which will exactly half-saturate the magnet. Dr. Frölich has independently made use of this conception, and has applied it to the formula of the electromagnet. The argument is his, but I retain the notation I have used.

Writing $(Si)'$ for the diacritical number of ampere-turns, we have (as I showed in 1884) $(Si)' = 1/\sigma$.

Taking the expression

$$H = \frac{G\kappa Si}{1 + \sigma Si} = \frac{G\kappa}{\sigma} \cdot \frac{Si}{\frac{1}{\sigma} + Si},$$

and writing

$$Y = \frac{G\kappa}{\sigma},$$

we have

$$H = Y \frac{Si}{Si + (Si)'}$$

where Y is obviously the limiting maximum value of H when the excitement is infinitely great. If S is given, then i' is the diacritical current, and the expression becomes

$$H = Y \frac{i}{i + i'},$$

which is true for every electromagnet excited by a single current. Two observations made on any electromagnet will determine the two constants Y and i'. Further, if r be the resistance of the magnetizing coil, since $ir = e$ (the potential requisite to send the current through the coil), we may obviously write the equation

$$H = Y \frac{e}{e + e'};$$

where e' is the diacritical difference of potential, namely that difference of potential which, applied to the coil of resistance r and of S convolutions, will half-saturate the core.

The extreme convenience of this form of the law of the electromagnet must be at once apparent, since it enables the equation of a given magnet to be instantly adapted to the case of any given current or potential, and is equally applicable to express either the intensity of the field or the magnetic moment of the magnet. To put the matter in a more general way, let ψ represent current, or potential, or ampere-turns, and let ψ'

be the diacritical value of the same for the given magnet ; let ϕ be the intensity of the field, or the strength of the pole, or the magnetic moment, or the integral of the magnetic induction, and Φ its maximum value ; then

$$\phi = \Phi \frac{\psi}{\psi + \psi'}.$$

This being the general equation of the electromagnet, it remains to be shown how excessively simple become the equations of the various kinds of dynamo.

3. *Equations of the Series-wound Dynamo.*

If A is the "equivalent area" of the coils of the armature, and H the average strength of the field in which it turns, the number of lines of force cut in each quadrant is AH ; hence the average electromotive force at the speed n is

$$E = 4n AH. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But

$$H = Y \frac{i}{i + i'},$$

and, writing $B = 4AY$, and remembering that, if ΣR be the sum of the resistances in the circuit, $E/\Sigma R = i$ (by Ohm's law), we get

$$i = \frac{nB}{\Sigma R} - i'.$$

But Y being the maximum value of H, it is obvious that nB is the maximum value that E could possibly have (at that speed) even if the magnets were separately excited to saturation. Hence $nB/\Sigma R$ is the maximum value that i could have if the magnets were thus separately saturated and the armature, driven at speed n, were in a circuit the total resistance of which was equal to ΣR. Adopting Frölich's notation here, we will write as $\bar{i}$ this current ; and as it is important to distinguish the current generated under such conditions, I propose to call it the "maximal" current*. The equation of the series dynamo now becomes

$$i = \bar{i} - i' ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or, multiplying each term by ΣR,

$$E = \bar{E} - E', \quad . \quad . \quad . \quad . \quad . \quad (3)$$

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* The *maximal* current must not be confused with the *maximum* current. The latter would be obtained by rotating the armature in the saturated field at a very high speed in a circuit of resistance so small that the current did just not fuse the conductors. The *maximal* current is that obtained at speed n in circuit of resistance ΣR when the magnets are separately excited to saturation.

where $\overline{\mathrm{E}}$ is the "maximal" value of E at that speed with saturated magnets. And, again, writing e for the difference of potentials at the terminals of the machine, since e, multiplied by the external resistance $\mathrm{R} = i$ under all circumstances, we have

$$e = \overline{e} - e'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It appears, then, that in the case of the series-wound dynamo, each of the single electrical quantities is equal to the difference between the "maximal" value which that quantity could have at that speed if the field-magnet were separately saturated and the "diacritical" value of the same quantity. This important result was announced by Frölich[*] in 1885.

It may be remarked that, since we may write

$$\mathrm{H} = \mathrm{Y}\,\frac{\mathrm{E}}{\mathrm{E} + \mathrm{E}'},$$

we may deduce from (1) the result

$$\mathrm{E} = n\mathrm{B} - \mathrm{E}',$$

and from this derive equation (3).

It may be noted, in passing, that B is the electromotive forces that would be generated in the armature at speed $= 1$ if the field-magnets were separately excited to absolute saturation. It is the maximal value of E at unit speed.

4. *Expressions for the "Dead Turns."*

It is known that in every dynamo the current (with a given resistance) is not proportional to the speed, but is proportional to the speed less a certain number of revolutions per second. This latter number is known familiarly as the "dead turns." It is also known that (with given resistance) there is a certain speed below which the dynamo does not excite itself. This least speed of excitement (with given resistance) is the same as the "dead turns." It is called by some the "critical" speed; though that name is preferably reserved for the speed that is critical for self-regulation, and which is (unlike the least speed of excitation) independent of the resistance.

We can find an expression for the dead turns as follows:—
Taking the expression for the current,

$$i = \frac{n\mathrm{B}}{\Sigma\mathrm{R}} - i'',$$

equate it to zero; the current of the machine (series-wound) being *nil* when n is reduced sufficiently to n' ($=$ the dead

[*] *Elektrotechnische Zeitschrift*, vi. p. 133, March 1885.

turns). Then

$$i' = \frac{n'B}{\Sigma R},$$

and

$$n' = \frac{i'\Sigma R}{B}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It appears, then, that the dead turns are proportional to the diacritical current and to the resistance; and inversely proportional to A and Y, the factors of B. It will be noted that $i'\Sigma R = E'$, the diacritical electromotive force.

Again, we have found

$$E = nB - E';$$

and as $E' = n'B$, it follows that

$$E = (n - n')B. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This result is interesting in itself, and might have been used as a starting-point for the equations of the dynamo, inasmuch as it is readily found by experiment as a fundamental relation between speed and electromotive force. Following the analogy and the nomenclature adopted, we may regard n' (the dead turns) as the diacritical speed. In other words, it is that speed at which, with magnets separately excited to saturation, the induced electromotive force will be diacritical, and will, with the given resistances ΣR, give the diacritical current. This equation (6) gives by far the best method of determining the important constant B. Two experiments to observe a pair of values of E and n will suffice to determine both n' and B.

5. *Equations of the Shunt Dynamo.*

Here we use r_s and i_s for the resistance and current of the shunt-coil, and r_a and i_a for those of the armature. We may then calculate the potential at terminals as follows. Writing $\mathbf{R}$ for the resistance of the whole system of machine and its circuits, as measured from brush to brush[*],

$$E = e + r_a i_a = e\frac{r_a}{\mathbf{R}} = 4n A H,$$

$$H = Y\frac{e}{e + e'},$$

$$e\frac{r_a}{\mathbf{R}} = nB\frac{e}{e + e'};$$

whence

$$e = \frac{nB\mathbf{R}}{r_a} - e'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

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[*] See my ' Dynamoelectric Machinery,' second edition, p. 298.

But $\dfrac{n\mathrm{B}\mathfrak{R}}{r_a}$ is the same thing as the "maximal" value of e which the machine would give, if the magnets were separately saturated, when working at the speed n through resistances as given; it may therefore be written as $\bar{e}$.

Whence, finally,

$$e = \bar{e} - e' . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

From which we get at once also for the shunt-current,

$$i_s = \bar{i}_s - i_s' ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and for the main-circuit current,

$$i = \bar{i} - i'' . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

6. *General Equation of the Self-exciting Dynamo.*

Let ψ be any one of the currents or potentials of the dynamo, $\overline{\psi}$ its "maximal" value, that is to say the value it has when the magnet is separately saturated, and ψ' its "diacritical value."

$$\overline{\psi} = f(n, \mathrm{A}, \mathrm{Y}, [\mathrm{R}]) ;$$

and, by the very nature of the case, whatever the form of the function f,

$$\psi = f(n, \mathrm{A}, \mathrm{H}, [\mathrm{R}]) ;$$

and

$$\mathrm{H} = \mathrm{Y} \, \frac{\psi}{\psi + \psi'} ;$$

whence

$$\psi = \overline{\psi} \, \frac{\psi}{\psi + \psi'} ;$$

and, finally,

$$\psi = \overline{\psi} - \psi' ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

which is the general equation of the self-exciting dynamo.

It may here be pointed out that the two terms on the right-hand side of this equation—the "maximal" and "diacritical" values of the quantity on the left—possess certain properties. In a given dynamo the "diacritical" term is a constant, whilst the "maximal" term is a variable which increases with the speed of driving. The maximal term when representing a current varies also with changes of resistance, in an inverse way, but differently in shunt dynamos from series dynamos: when it represents an electromotive force it does not vary with changes of resistance. A year ago Dr. Frölich sought to divide the equation of the dynamo into two parts—an

armature part and a field-magnet part.   This does not quite correspond to the case.   The " diacritical " term appertains to field-magnet primarily ; but since the number of ampere-turns of current that will produce a half-saturated magnetic field depends also on the quantity and quality of the iron in the machine, it is impossible to regard it as independent of the iron masses of the armature.   The " maximal " term is proportional both to A, the total effective area of the armature-coils, and to Y, the maximum value of the magnetic field.   The " diacritical " term for currents is lowered by increase in the number of magnetizing coils, and for potentials is raised by increase in the resistance of the magnetizing coils.   The " maximal " term is not altered by altering the magnetizing coils, but increases with an increase in the number of coils of the armature, and, for currents, decreases with increasing resistance. If the iron parts of a dynamo be given of a certain form, size, and quality, then it may in general be said that ψ' depends only on the windings (and, for currents, on the resistance) of the field-magnet, and $\bar{\psi}$ depends only on the windings of the armature (and, for currents, on its resistance) and not upon the windings of the field-magnet, or on their resistance except (and this only for currents) so far as their resistance contributes to the resistance of the whole circuit through which the current generated in the armature flows.

7. *Conditions of Self-regulation for Compound-wound Dynamos.*

In this case we write the formula of the electromagnet in terms of the ampere-turns ; Si_a for the excitation due to the coils in the armature part of the circuit, Zi_s for that due to the shunt, and ϕ' as the diacritical number of ampere-turns. Then, writing

$$E = 4n A H ;$$

$$e = E - (r_a + r_m)i_a ;$$

$$H = Y \frac{Zi_s + Si_a}{Zi_s + Si_a + \phi'} = Y \frac{Zi_s + Si_a}{\bar{\phi}}, \text{ by equation (11) } ante ;$$

$$B = 4AY ;$$

we have

$$e\phi' = i_s Z(nB - e) + i_a \{S(nB) - e) - (r_a + r_m)\bar{\phi}\}. \quad . \quad (12)$$

The condition sought is to be such as to make e constant. Now ϕ' is a constant, so is i_s if e is, and n may be made constant at any value we please.   Hence, as i_a is a variable, the condition of constancy can only be attained by giving the

dynamo such a speed n_1 that the coefficient of i_a shall be zero, or that

$$S(n_1B-e) = (r_a+r_m)\overline{\phi}. \quad . \quad . \quad . \quad (13)$$

But $\overline{\phi}$, the maximal number of ampere-turns, is not itself a constant, since it contains as one of the three terms in its sum the quantity Si_a. Hence we gather that absolute self-regulation is physically impossible; and it approaches to perfection as $Zi_s+\phi'$ are great as compared with Si_a. In other words, there must be so much iron in the machine that the diacritical excitement is very great, and it must have a small armature-resistance; otherwise S (and Si_a) cannot be small as compared with Z (and Zi_s). This is known already to electric engineers. Assuming that the dynamo is well designed in these respects, $\overline{\phi}$ will be very nearly constant, and the equation of condition may be accepted as adequately true. This leaves equation (12) in the form

$$e\phi' = i_sZ(n_1B-e),$$

$$\frac{e\phi'}{i_sZ} = n_1B-e,$$

$$\frac{r}{Z}\phi' = n_1B-e.$$

Putting in this value of n_1B-e into equation (13), we have

$$\frac{r_s}{Z}\phi' = \frac{r_a+r_m}{S}\overline{\phi}. \quad . \quad . \quad . \quad . \quad (14)$$

Now ϕ' may be written either as Si_a' or as Zi_s', and $\overline{\phi}$ may be written either as $S\overline{i_a}$ or as $Z\overline{i_s}$. Choosing the second form in the first case and the first form in the second case, we may obtain

$$r_s i_s' = (r_a+r_m)\overline{i_a};$$

or, finally,

$$e_s' = \overline{e_{a+m}}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or, the diacritical value of the potential at terminals for the shunt-wound part of the circuit must be equal to the maximal value of the potential at terminals for the armature and series-wound part. The equation (14) also gives

$$\frac{Z}{S} = \frac{r_s}{r_a+r_m} \cdot \frac{\phi'}{\overline{\phi}}, \quad . \quad . \quad . \quad . \quad (16)$$

which is more correct than the formula usually given hitherto for the ratio of the shunt- and series-windings, and which assumes absence of saturation terms.

The simplicity of these results, no less than that of the processes by which they are derived, lends additional value to the new formula of Dr. Frölich, whose work deserves to be more widely known and recognized than it now is.

City and Guilds of London Technical College,
    Finsbury, June 1886.

XXXV. *Electromagnets.*—V. *The Law of similar Electromagnets.* *Saturation, &c.* By R. H. M. BOSANQUET, *St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN previous papers on Electromagnets I have (1)* indicated the general nature of a formula for the moment and induction in electromagnets, and in subsequent papers † given the details of the complete examination of the permeabilities of many specimens of iron and steel, together with an attempt at a molecular formula for these permeabilities worked out in some detail. In the present communication I propose to give as shortly as possible an abstract of the results of a great number of experiments having special reference to the magnetic resistance of cylindrical bars of length equal to twenty times their diameter, with and without pole-pieces.

The datum in question (magnetic resistance) is that needed to define the magnetism under given electromagnetic excitation.

The experiments cover the whole region from small magnetic inductions up to saturation, or what would be commonly called so.

The experiments have been made on bars of different sizes of the proportions in question, so as to furnish for the first time an experimental examination of the law of the magnetism of similar solids. It appears to be of great interest to ascertain how far this law can be depended upon in practice.

The result is that, while in the main the law is conformed to, the irregularity shown by different specimens, especially in the extreme regions of small inductions and saturation, is very great. General deductions therefore, such as have been recently published, depending on the behaviour of single

* Phil. Mag. xvii. p. 531.   † Ibid. xix. pp. 73, 333; xx. p. 318.

specimens, cannot be accepted as having more demonstrative force than belongs to preliminary investigations. So far as work of this description goes, my own paper in Phil. Mag. June 1884 (xvii.) p. 531, showed in detail the properties of bars ; and specially pointed out that the course of the values did not at all indicate any definite limit to the magnetism, and that the existing idea of saturation was a complete mistake (p. 535). This is now being published as a new discovery.

In subsequent papers I have shown that rings do not exhibit the same behaviour ; and though some ring specimens admit of the magnetism being forced up very high, yet others do not.

Further, where the magnetism is forced up very high in rings, the whole magnetization function appears to change under the influence ; and the values thus obtained are not generally capable of being satisfied by the same expression, which represents the behaviour of the specimen under inductions less than $\mathfrak{B} = 18,000$.

The difference in this respect between rings and bars is already indicated in my paper, Phil. Mag. 1885 (xix.) p. 79 ; and it is there pointed out that the cause is probably the difference in distribution, *i. e.* that in bars the lines of force are crowded closely only at the equatorial section.

The law of similar solids is easily deduced from the fact that magnetic resistance (quotient of potential by induction) is of linear dimensions. Hence, in similar electromagnets, where the inductions are the same, the number of current turns required is proportional to the linear dimension[*].

The present two series of experiments were made on cylindrical bars having the following dimensions:—The pole-pieces of the second set were circular, had a diameter five times that of the bar, and thickness equal to the diameter of the bar. The object of the experiments on the bars with pole-pieces was mainly to obtain numbers which might assist to serve as bases for a knowledge of the behaviour of the field-magnets of dynamos.

[*] I have long been under the impression that this law was enunciated in substance by Sir W. Thomson. But after carefully going through the reprinted papers, the nearest I could find to it is at p. 435 ('Electrostatics and Magnetism'). But this statement does not include the proposition that the number of current-turns is proportional to the linear dimensions. And I am therefore doubtful whether the law is really due to Sir W. Thomson.

The linear scale is varied in proportions up to 1 : 5. The largest of the masses weighed considerably over a cwt., and taxed our resources to handle it.

Dimensions of Bars.

	PLAIN BARS.				BARS WITH SOLID P.P. (pole-pieces).		
Bars.	Length, centim.	Radius, centim.	n coils.	Bars.	Length, centim.	Radius, centim.	n coils.
I.	20	0·5	1092	I.	20·6	0·5	1121
II.	20	0·5	1109	II.	20·7	0·5	1110
IV.	40	1·0	1933	III.	38·08	1·0	2024
V.	40	1·0	1931	VI.	38·08	1·0	2134
VII.	60	1·5	1951	XVII.	57·5	1·5	2837
VIII.	60	1·5	1909	XVIII.	57·0	1·5	2813
IX.	80	2·0	2557	XV.	73·9	2·04	3546
X.	80	2·0	2543	XVI.	73·9	2·04	3574
XI.	100	2·5	3187	XIII.	94·85	2·5025	3041
XII.	100	2·5	3172	XIV.	94·8	2·525	3028

The determinations of bar II. with pole-pieces were puzzling and anomalous, and were excluded. Determinations for small inductions were in all cases made in the first instance by reversing the effect of the horizontal component of the earth's magnetism. The bars were then wound uniformly from end to end, and the inductions determined by the effect on coils placed equatorially.

In many cases great discrepancies occurred even between bars of the same measurement (they were all in pairs). In these cases the determinations were repeated, and in several cases the whole of the magnetizing coils were rewound.

The results were then plotted on a large figure for each bar, and a curve was drawn by hand to represent the values as well as possible ; 𝕭 was abscissa, and ρ (magnetic resistance) ordinate.

It was at once evident that in all cases, almost without exception, the curve did not show $\rho = \infty$ for any finite value of 𝕭, but rather indicated the existence of an asymptote inclined at a moderate angle to the axis of ρ.

The values of ρ corresponding to 𝕭 = 0, 1000, 2000, &c. were then measured off, and all divided by the lengths of the bars, so as to reduce them to data for bars 1 centim. long, according to the above law of similarity.

These values, with the resulting means, form the two following Tables.

Plain Bars.

Table of ρ for length = 1 centim.

$\mathfrak{B}=$	0,000	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000	9,000	10,000	11,000	12,000	13,000	14,000	15,000	16,000	17,000	18,000
Means.	·00990	·00660	·00592	·00582	·00576	·00573	·00571	·00570	·00571	·00572	·00577	·00580	·00587	·00601	·00621	·00661	·00730	·00866	·01064
XII.	·01203	·00755	·00576	·00568	·00567	·00567	·00564	·00565	·00566	·00568	·00572	·00574	·00580	·00587	·00600	·00618	·00660	·00740	
XI.	·00770	·00621	·00589	·00590	·00588	·00589	·00588	·00588	·00590	·00593	·00600	·00602	·00617	·00634	·00654	·00675	·00712	·00770	
X.	·00975	·00706	·00627	·00625	·00625	·00625	·00625	·00625	·00625	·00625	·00631	·00641	·00662	·00715	·00766	·00875	·01056	·01300	·01552
IX.	·00937	·00687	·00625	·00617	·00616	·00615	·00615	·00615	·00615	·00620	·00622	·00625	·00625	·00654	·00720	·00831	·00975	·01162	·01375
VIII.	·01000	·00595	·00533	·00513	·00517	·00520	·00530	·00533	·00542	·00547	·00552	·00563	·00567	·00577	·00588	·00605	·00647	·00790	·01050
VII.	·00905	·00590	·00550	·00550	·00547	·00547	·00547	·00547	·00550	·00550	·00560	·00562	·00570	·00577	·00583	·00612	·00705	·00957	
V.	·01100	·00650	·00570	·00560	·00545	·00545	·00545	·00545	·00545	·00545	·00545	·00545	·00550	·00555	·00562	·00582	·00610	·00650	·00750
IV.	·01000	·00650	·00592	·00587	·00580	·00575	·00575	·00570	·00565	·00562	·00570	·00570	·00575	·00590	·00608	·00675	·00780	·00970	·01340
II.	·00800	·00735	·00680	·00645	·00615	·00590	·00570	·00560	·00560	·00560	·00560	·00565	·00565	·00565	·00570	·00570	·00575	·00615	·00685
I.	·01200	·00610	·00510	·00570	·00560	·00555	·00555	·00555	·00555	·00555	·00555	·00555	·00555	·00555	·00555	·00565	·00595	·00640	·00700

Bars with solid P.P. (pole-pieces).

Table of ρ for length = 1 centim.

ℬ =	0,000	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000	9,000	10,000	11,000	12,000	13,000	14,000	15,000	16,000	17,000	18,000
I.	·00582	·00350	·00306	·00291	·00291	·00291	·00291	·00291	·00291	·00296	·00301	·00301	·00311	·00330	·00374	·00495	·00689	·00995	·01442
III.	·00788	·00428	·00383	·00354	·00315	·00297	·00280	·00280	·00280	·00280	·00280	·00280	·00289	·00297	·00307	·00341	·00394	·00620	·01024
VI.	·01840	·00761	·00420	·00302	·00289	·00289	·00289	·00289	·00289	·00289	·00291	·00294	·00302	·00312	·00323	·00355	·00436	·00630	·00982
XVII.	·00428	·00875	·00323	·00305	·00293	·00288	·00286	·00284	·00284	·00284	·00284	·00286	·00288	·00295	·00318	·00360	·00447	·00719	·01053
XVIII.	·00575	·00351	·00330	·00318	·00305	·00296	·00291	·00289	·00288	·00289	·00296	·00305	·00316	·00337	·00400	·00526	·00842		
XV.	·00609	·00373	·00342	·00325	·00314	·00311	·00311	·00311	·00311	·00313	·00315	·00317	·00327	·00348	·00376	·00415	·00555	·00785	
XVI.	·00704	·00380	·00340	·00311	·00304	·00302	·00302	·00302	·00302	·00302	·00302	·00302	·00302	·00302	·00303	·00356	·00480	·00656	·01015
XIII.	·00685	·00329	·00313	·00307	·00304	·00304	·00304	·00304	·00306	·00309	·00313	·00323	·00334	·00353	·00388	·00501	·00796		
XIV.	·00775	·00369	·00322	·00302	·00295	·00295	·00295	·00295	·00296	·00297	·00298	·00306	·00316	·00330	·00354	·00432	·00627		
Means.	·00743	·00413	·00342	·00313	·00301	·00297	·00294	·00294	·00294	·00295	·00298	·00301	·00309	·00323	·00349	·00420	·00585	·00734	·01103
	·00593	·00369	·00332 numbers adopted.																

It will be noticed that the numbers of windings are not exactly proportional to the linear dimensions. It was not practicable to carry this out ; and of course it does not matter whether a given number of current-turns is made up one way or the other (by current or turns).

In the first three entries of the bars with pole-pieces, the initial discrepancies being very great, the means of selected values were adopted.

The discrepancies between the numbers in any one column are those in which any deviation from the law of similars would appear. While the values (particularly at beginning and end) are capricious in the extreme, it is certainly not possible to detect any systematic difference between the numbers belonging to the large and small bars.

Before proceeding to calculate with these numbers, I will now exhibit in a small figure the nature of the relation between the magnetic resistances due to

(1) the permeability of the metal, as derived from rings ;
(2) the bars with pole-pieces (P.P.); and
(3) plain cylindrical bars.
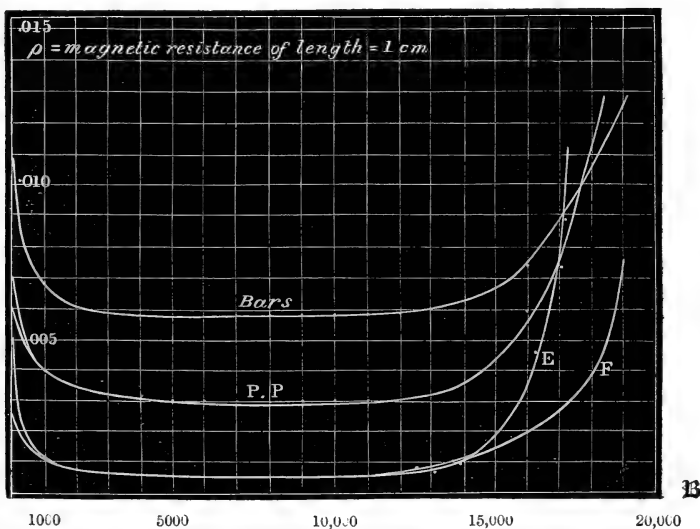
The resistance in each case is supposed to be due to a length of metal = 1 centim., and ρ is expressed in decimals of a centimetre, though drawn to a different scale for convenience. The lowest curve is made up of the values from rings E, F

(Phil. Mag. xix. p. 76), so as to show the wide limits within which specimens vary. The middle curve is that of the bars with pole-pieces. The initial mean and adopted value are both traced. The upper curve is that of the plain bars.

This figure illustrates, and indeed proves, for approximate purposes the rule I enunciated (Phil. Mag. xvii. p. 534), and shows how the rule includes bars with pole-pieces, and where it fails, viz. in the region spoken of as in the neighbourhood of saturation. The rule is :—" The magnetic resistance of any bar can be expressed as the sum of a resistance due to its form, and a quantity formed by dividing the length by μ, the permeability of the metal."

In the figure the lowest curve represents the resistance due to the imperfect permeability of the metal. The two upper curves obviously admit of derivation from the lower one by addition of constants due to the respective shapes of the bars, allowing for the capriciousness of the values in different specimens, and for the difference in the region of saturation ($\mathfrak{B} = 18,000$ and thereabouts).

As to the amount:—The analogy of the resistance to a fluid flowing from the end of a pipe would give $2 \times \cdot 6$ of the radius, or in the present case $\cdot 012$ centim., for the shape-resistance. The fact that magnetism issues all along the bar, more or less, diminishes this resistance ; so that we find in fact that in the plain bar the addition for shape is somewhat over $\cdot 005$ centim., as we can see in the figure.

Again, the pole-pieces still further facilitate the flux of the magnetic induction through the ends, and the shape-resistance in this case is somewhat over $\cdot 002$ centim.

I proceed to a more detailed examination of the numbers.

For the plain bars, the shape-resistance calculated from the formula at Phil. Mag. xvii. p. 534 exactly satisfies the requirements of the numbers. Hence the first step is to subtract the value thus calculated from the number to be dealt with ; so that $\cdot 37 \, r \, 10^{-\cdot 006) \frac{l}{r}} = \cdot 00528$ is subtracted, and $\rho - \cdot 00528 = \dfrac{1}{\mu}$

gives the calculated values of μ. These are then fitted to formulæ in accordance with the theory of Phil. Mag. xix. p. 92. With one exception, which is as follows :—Since the saturation-curve approximates to an inclined asymptote, it is impossible that $\mathfrak{B}$ can have finite maximum values ; and the best way of making allowance for this has appeared to be, to substitute for the definite maximum $\mathfrak{B}_\infty$, a sum made up of a constant B and a term proportional to the magnetizing

force, whose coefficient was determined from the observed values.

In the figure which follows, the crosses show the observed values ; the dotted line shows the best representation by means of a maximum value of the induction $\mathfrak{B}_\infty$, fitted to the highest value ; and the dashed line shows the representation by means of the constant B and the term proportional to the magnetizing force.
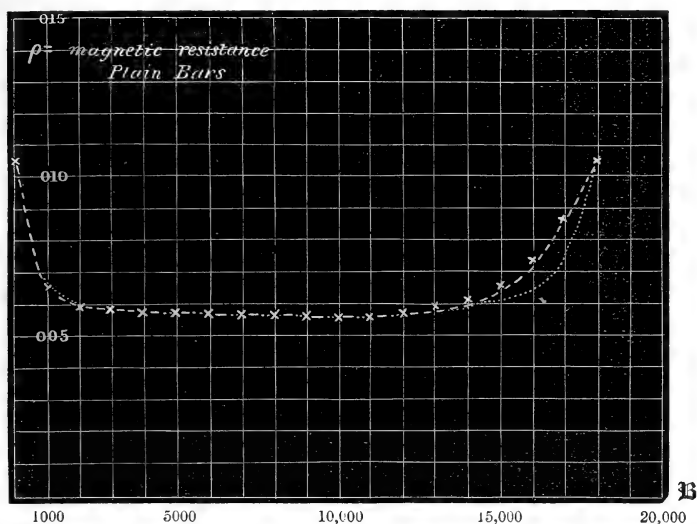
The two following Tables give the complete numerical results of the representation of the numbers for plain bars ; first, according to the calculation with a fixed maximum induction, and, secondly, where the maximum induction is made to contain a term proportional to the magnetizing force. The extraordinary gain in the representation of the saturation region in the latter case will be noticed.

$\dfrac{\mathfrak{B}}{\mu}$ is the magnetizing force. Its coefficient m in the second table is 18·5 nearly.

Plain Bars without Magnetizing-Force term.

ρ calc. $= \cdot 00528 + \dfrac{l}{\mu}$ $l = 1$ centim., $\log \dfrac{\kappa}{\rho} = 2 \cdot 34670$,

$\mu = \cdot 395 (18593 - \mathfrak{B}) \cos \delta$, $\log f = \cdot 16896$.

$$\delta = f\theta, \quad \mathfrak{B} = \frac{\kappa}{\rho} \; \frac{60° - \omega\theta}{\sin \theta}.$$

$\mathfrak{B}$.	μ.		Diffs.	ρ.		Diffs.
	Calc.	Obs.		Calc.	Obs.	
0,000	188	188	0	·01060	·01061	− ·00001
1,000	845	806	+ 39	·00646	·00652	− ·00006
2,000	1366	1562	−196	·00601	·00592	+ ·00009
3,000	1766	1852	− 86	·00585	·00582	+ ·00003
4,000	2052	2083	− 31	·00577	·00576	+ ·00001
5,000	2239	2222	+ 17	·00573	·00573	·00000
6,000	2343	2326	+ 17	·00571	·00571	·00000
7,000	2376	2381	− 5	·00570	·00570	·00000
8,000	2346	2326	+ 20	·00571	·00571	·00000
9,000	2264	2273	− 9	·00572	·00572	·00000
10,000	2139	2041	+ 98	·00575	·00577	− ·00002
11,000	1979	1923	+ 56	·00579	·00580	− ·00001
12,000	1785	1695	+ 90	·00584	·00587	− ·00003
13,000	1566	1370	+196	·00592	·00601	− ·00009
14,000	1324	1075	+249	·00604	·00621	− ·00017
15,000	1062	752	+310	·00622	·00661	− ·00039
16,000	784	495	+289	·00656	·00730	− ·00074
17,000	491	296	+195	·00732	·00866	− ·00134
18,000	186	186	0	·01065	·01064	+ ·00001

Plain Bars with Magnetizing-Force term.

ρ calc. $= \cdot 00528 + \dfrac{l}{\mu}$, $l = 1$ centim., $\log \dfrac{\kappa}{\rho} = 2 \cdot 54133$,

$\mu = \cdot 65 \left(\dfrac{n\mathfrak{B}}{\mu} + 16,640 - \mathfrak{B} \right) \cos \delta$, $\log f = \cdot 17126$,

$\log n = 1 \cdot 26665$.

$$\delta = f\theta, \quad \mathfrak{B} = \frac{\kappa}{\rho} \; \frac{60° + \omega}{\sin \theta}.$$

$\mathfrak{B}$.	μ.		Diffs.	ρ.		Diffs.
	Calc.	Obs.		Calc.	Obs.	
0,000	189	188	+ 1	·01057	·01061	− ·00004
1,000	813	806	+ 7	·00651	·00652	− ·00001
2,000	1326	1562	−236	·00603	·00592	+ ·00011
3,000	1731	1852	−121	·00586	·00582	+ ·00004
4,000	2028	2083	− 55	·00577	·00576	+ ·00001
5,000	2235	2222	+ 13	·00573	·00573	·00000
6,000	2355	2326	+ 29	·00570	·00571	− ·00001
7,000	2394	0381	+ 13	·00570	·00570	·00000
8,000	2391	2326	+ 65	·00570	·00571	− ·00001
9,000	2271	2273	− 2	·00572	·00572	·00000
10,000	2123	2041	+ 82	·00575	·00577	− ·00002
11,000	1924	1923	+ 1	·00580	·00580	·00000
12,000	1683	1695	− 12	·00587	·00587	·00000
13,000	1409	1370	+ 39	·00599	·00601	− ·00002
14,000	1108	1075	+ 33	·00618	·00621	− ·00003
15,000	801	752	+ 49	·00653	·00661	− ·00008
16,000	509	495	+ 14	·00724	·00730	− ·00006
17,000	297	296	+ 1	·00864	·00866	− ·00002
18,000	186	186	0	·01065	·01064	+ ·00001

The representation of the numbers derived from the bars with pole-pieces affords results of a similar character. It is not necessary to present the figure showing the nature of the gain in accuracy by introduction of the term proportional to the magnetizing force, as it is precisely similar in character to that already given for plain bars. The two following Tables give the representation of these numbers in the same way as the two last. The coefficient of the magnetizing force in the second table is 12·1 nearly.

Bars with P.P. (pole-pieces) without Magnetizing-Force term.

$$A + \cdot 415 \, (18366 - \mathfrak{B}) \cos \delta, \qquad \log \frac{\kappa}{\rho} = 2 \cdot 35933,$$

$$\log f = \cdot 16500.$$

$\mathfrak{B}$.	μ.		Diffs.	ρ.		Diffs.
	Calc.	Obs.		Calc.	Obs.	
0,000	290	293	− 3	·00596	·00593	+ ·00003
1,000	905	855	+ 50	·00362	·00369	− ·00007
2,000	1397	1250	+147	·00324	·00332	− ·00008
3,000	1772	1639	+133	·00308	·00313	− ·00005
4,000	2046	2041	+ 5	·00301	·00301	·00000
5,000	2225	2222	+ 3	·00297	·00297	·00000
6,000	2323	2381	− 58	·00295	·00294	+ ·00001
7,000	2352	2381	− 29	·00294	·00294	·00000
8,000	2323	2381	− 58	·00295	·00294	+ ·00001
9,000	2240	2326	− 86	·00297	·00295	+ ·00002
10,000	2115	2174	− 59	·00299	·00298	+ ·00001
11,000	1952	2041	− 89	·00303	·00301	+ ·00002
12,000	1756	1754	+ 2	·00309	·00309	·00000
13,000	1532	1408	+124	·00317	·00323	− ·00006
14,000	1285	1031	+254	·00330	·00349	− ·00019
15,000	1051	595	+456	00347	·00420	− ·00073
16,000	733	300	+433	·00388	·00585	− ·00197
17,000	432	207	+225	·00484	·00734	− ·00250
18,000	118	118	0	·01100	·01103	− ·00003

Bars with P.P. (pole-pieces) with Magnetizing-Force term.

$$\mu = A = 69 \left(\frac{n\mathfrak{B}}{\mu} + B - \mathfrak{B} \right) \cos \delta,$$

$$\log n = 1 \cdot 08160,$$
$$\log \frac{\kappa}{\rho} = 2 \cdot 57346,$$
$$\log f = \cdot 16878.$$

$\mathfrak{B}$.	μ.		Diffs.	ρ.		Diffs.
	Calc.	Obs.		Calc.	Obs.	
0,000	297	293	+ 4	·00589	·00593	− ·00004
1,000	894	855	+ 39	·00364	·00369	− ·00005
2,000	1384	1250	+134	·00324	·00332	− ·00008
3,000	1767	1639	+128	·00309	·00313	− ·00004
4,000	2056	2041	+ 15	·00301	·00301	·00000
5,000	2250	2222	+ 28	·00296	·00297	− ·00001
6,000	2358	2381	− 23	·00294	·00294	·00000
7,000	2389	2381	+ 8	·00234	·00294	·00000
8,000	2351	2381	− 30	·00294	·00294	·00000
9,000	2249	2326	− 77	·00296	·00295	+ ·00001
10,000	2091	2174	− 83	·00300	·00298	+ ·00002
11,000	1882	2041	−159	·00305	·00301	+ ·00004
12,000	1627	1754	−127	·00313	·00309	+ ·00004
13,000	1339	1408	− 69	·00327	·00323	+ ·00004
14,000	1021	1031	− 10	·00350	·00349	+ ·00001
15,000	707	595	+112	·00393	·00420	− ·00027
16,000	451	300	+151	·00473	·00585	− ·00112
17,000	179	207	− 28	·00810	·00734	+ ·00076
18,000	118	118	0	·01102	·01103	− ·00001

Many rings have been completely examined in my various papers. It is a matter of interest to what extent these support the position that there is an absolute saturation limit, and to what extent they oppose it.

First, as to Rowland's own measures of the one ring which he examined completely. I have reduced them according to my formula at Phil. Mag. xix. p. 340. Although the magnetism is not carried very high, and the fitting has not been as perfectly accomplished as in my more recent calculations, yet it is clear that there is no indication of variation of the saturation limit. This generally shows itself in an excess of the calculated values of μ in the region $\mathfrak{B} = 15,000$ –16,000.

In all my own determinations the magnetism has been carried up much higher; and I think in some of the rings discussed at Phil. Mag. xix. pp. 76–79 there is some appearance of this kind. But I have repeatedly submitted nearly all of these to calculation, with the result that the introduction of the variable saturation limit, so as to fit the region in question, invariably

unsettles the general representation of the values. So that in every case, without exception, the general representation of the behaviour of rings, furnished by calculation on the basis of a fixed saturation limit, has been far better than any obtained by using a variable saturation limit. At the same time it must be admitted that experiment sometimes shows a heightening of the apparent saturation limit as it is approached. The change, however, resembles rather a change in the properties of the metal than a continuation according to the same laws which successfully represent the general course of the values.

I must reserve for another occasion some considerations as to dynamo machines, founded on the numbers here obtained for bars with pole pieces.

It must be noticed that the bars here dealt with are only of one definite shape, viz. diameter : length : : 1 : 20 or thereabouts. And the object of the investigation was to get a series of reliable data with respect to this one shape, which might afford a sound starting-point for the investigation of other proportions.

XXXVI. *Intelligence and Miscellaneous Articles.*

MEASUREMENT OF PITCH BY MANOMETRIC FLAMES.
BY M. DOUMER.

MANOMETRIC flames have hitherto only been used as a means of demonstration, and for studying the quality of vowel sounds. They are, however, susceptible of more varied applications, and may in particular vie with the graphical method in studying the height of sounds.

For this purpose two adjacent manometric flames are taken, one of which is due to a sound of known height, the other to a sound the height of which is to be determined, and then by means of a rotating mirror it is ascertained how many vibrations of the sound under investigation correspond to a known number of vibrations of the chronograph. A simple proportion gives the desired height.

This method, which is very simple in theory, is in practice complicated, and almost impossible from the want of centering of the mirror. But it retains all its simplicity, and a certain elegance moreover, if we substitute a sensitive plate for the rotating mirror ; in other words, if the two manometric flames are simultaneously photographed on the same plate.

The camera obscura used is a broad one, provided with a lens of

very short focus, and with a suitable shutter devised by M. Duboscq. The focussing is easily effected, either by moving the object-glass, or, better, by moving the manometric flames.

The motion of the flames to be photographed is so rapid that we should have recourse to the most sensitive plates. Those of Monckhoven have given very good results; I prefer, however, the very sensitive plates prepared with silver iodide by Frank's formula.

But whatever be the plates used, the negatives will always be too weak if we do not take the precaution of using a lens of short-focus, and of making the flames as bright as possible. This is attained if we carbonize the gas by passing it through pumicestone impregnated with benzole, and burning it in pure oxygen. By suitably regulating the supply of gas and oxygen, we obtain a flame of great lustre.

The plates are developed by the ordinary photographic methods; they have then two rows of parallel dentations, one of which corresponds to the vibrations of the chronographic flame, and the other to the vibrations of the flame worked by the sound whose height is to be measured.

The comparison of the two flames is then an easy matter. It may be made in two ways; either by the measurement of the number of vibrations and fractions of vibrations comprised within equal lengths, or by the determination of the space occupied by known whole numbers of vibrations.

If the height of the flame has been suitably adjusted, so as to give images of 1·5 to 2 millim., these measurements are made with great facility and remarkable precision.

This method, devised for the purpose of lengthened researches on the vowel sounds, has been verified with great care for the sounds corresponding to the scales Ut_1, Ut_2, Ut_3, and Ut_4 by the aid of the chronograph Ut_3, of sliding diapasons, and of open pipes constructed by M. König with his usual care.

The following table shows the certainty and accuracy of the method, since the distances between the heights found and the heights indicated do not exceed a double vibration:—

Number of plates.		Note.	Heights.	
			Measured.	Indicated.
99	. . . .	Re_2	287·88	288
102	. . . .	Mi_4	1280·00	1280
103	. . . .	Ut_2	256·20	256
100	. . . .	Ut_4	1022·50	1024
101	. . . .	Sol_3	767·10	768

To measure very high or very low sounds, it is good to have recourse to two chronographic diapasons, one giving 100 vibrations per second, and the other about 2000. In this case in fact, where the difference of height between the two sounds is too considerable,

the velocity to be given to the plate to photograph the most acute sound spreads out the image of the deeper one, and the measurement becomes very difficult.—*Comptes Rendus,* August 2, 1886.

ON A NEW METHOD FOR DETERMINING THE VERTICAL INTENSITY OF A MAGNETIC FIELD. BY R. KRÜGER.

The methods hitherto used for measuring the vertical intensity of a magnetic field depend on the electromotive action which it exerts on a spiral which rotates about a horizontal axis. If, moreover, the spiral turns about a vertical axis, the ratio of the currents induced in the two rotations gives the inclination of the magnetic lines of force to the horizon. If, again, the current induced by a rotation about a horizontal axis be determined in absolute measure by a galvanometer, the vertical intensity of the field is determined from the contents of the surface enclosed by the windings of the spiral, and from the absolute resistance of the circuit formed by the spiral and the galvanometer. In a field of small extent the rotation must be replaced by a parallel displacement of the spiral in its plane. In any case the determination of a vertical intensity by these means is a difficult and tedious operation.

In contrast with this, the deflection which a disk suspended horizontally by means of a vertical wire in a solution of copper sulphate experiences when traversed in a radial direction by a current, forms a very convenient means of determining the vertical magnetic force which produces that deflection.

In order to test the capability of this method, it was used to determine the vertical intensity of the terrestrial magnetism, or rather the magnetic inclination.

The value of the vertical intensity was thus found to be

$$V = 2 \cdot 2903 \times T,$$

in which T is the horizontal intensity. Simultaneous observations with the terrestrial inductor gave

$$V = 2 \cdot 2899 \times T;$$

while the vertical intensity deduced from the formula for variations given by Prof. Schering[*] would be

$$V = 2 \cdot 2895 \, T.$$

Taking as a mean,

$$V = 2 \cdot 2899 \, T,$$

[*] *Gött. Nach.* 1882, p. 388.

it will be seen that the divergence obtained by this method does not exceed $\frac{1}{5700}$.

The rest of the paper contains the establishment of the formula, and the experimental details and data.—Wiedemann's *Annalen*, vol. xxviii. p. 613 (1886).

ON THE CONSTANT OF THE SUN'S HEAT.   BY M. MAURER.

The author compares the values for this constant, obtained previously by Pouillet, Hagen, Crova, Violle, and Langley; those given by Violle and Langley appear too great.   This seems to arise from the fact that Langley and Violle have found too high a value for the amount of the solar radiation on the Earth's surface, while the methods of other observers, to which those of Röntgen and Exner, and Desains must be added, agree very well with each other.

Actinometric measurements have recently been made with a new apparatus of F. Weber's, under remarkably good atmospheric conditions.   Six of them were made on the terrace of the Polytechnicum at Zürich, two on the top of the St. Gothard pass (2100 metres), and one on the Pizzo Centrale (3000 metres).   According to these, the maximum heat from the Sun at midday, which a surface of a square centimetre receives in a minute under perpendicular radiation, is :—

In Zurich	1·10—1·32 thermal unit.
On the St. Gothard ...	1·38—1·41 ,, ,,
,, Pizzo Centrale..	1·52 ,, ,,

These agree very well with the above observations, excepting for Violle's and Langley's.—*Zeitschrift der Oesterr. Gesellschaft. für Meteorologie*, vol. xx. p. 296, 1885 ; *Beiblätter der Physik*, vol. x. p. 182.

ON THE INCREASE OF TEMPERATURE PRODUCED BY A WATERFALL.   BY M. KELLER.

The slight increase of temperature in a waterfall is powerfully affected by the sources of error which the author discusses.   His observations at the waterfall at Terni should have given a difference of temperature of 0°·37 ; the measured ones varied between 0°·08 and 0°·73.   The author considers these numbers an evidence of the transformation of *vis viva* into heat in this waterfall.—*Atti della R Acc. dei Lincei* [4] i. pp. 671–676 (1885); *Beiblätter der Physik*, vol. x. p. 333.

XXXVII. *Turbines.* By J. LESTER WOODBRIDGE, *B.S., M.E.*[*]

I PROPOSE to discuss the action of turbines in general. Consider first the water after it has entered the wheel and is passing along its vanes. Conceive the water to be divided into an infinite number of filaments by vanes similar to those of the wheel, but subjected to the condition that, at each point, their width, *ac* (fig. 1), measured on the arc whose centre is O, shall subtend at the centre a constant angle $d\theta$. Conceive each filament to be divided into small prisms whose bases are represented by the shaded areas $a'b'c'd'$, $d'c'c''d''$, and *abcd*, by vertical planes normal to the vanes, making the divisions *ae*, *ef*, intercepted on the radius by circles passing through the consecutive vertices on the same vane a', d', d'', &c., equal. The variable height of a prism represent by x, and let ρ be the variable distance from the centre.

Then $d\rho = ae,\ ef,$ &c.

$\rho d\theta d\rho = abcd,$ &c. $=$ area of the base of an infinitesimal prism ;

$x\rho d\theta d\rho =$ volume of infinitesimal prism ;

$x\delta\rho d\theta d\rho = m =$ the mass of prism, δ being its density ;

$\gamma = san =$ angle between the normal to the vane at any point ρ, and the radius O*a* prolonged through that point;

$v =$ the velocity of a particle along the vane at ρ ;

$\omega =$ the uniform angular velocity of the wheel ; and

$p =$ the pressure of the water at the point ρ.

* Communicated by De Volson Wood, Professor of Engineering in Stevens Institute of Technology, Hoboken, N.J.

Fig. 1.
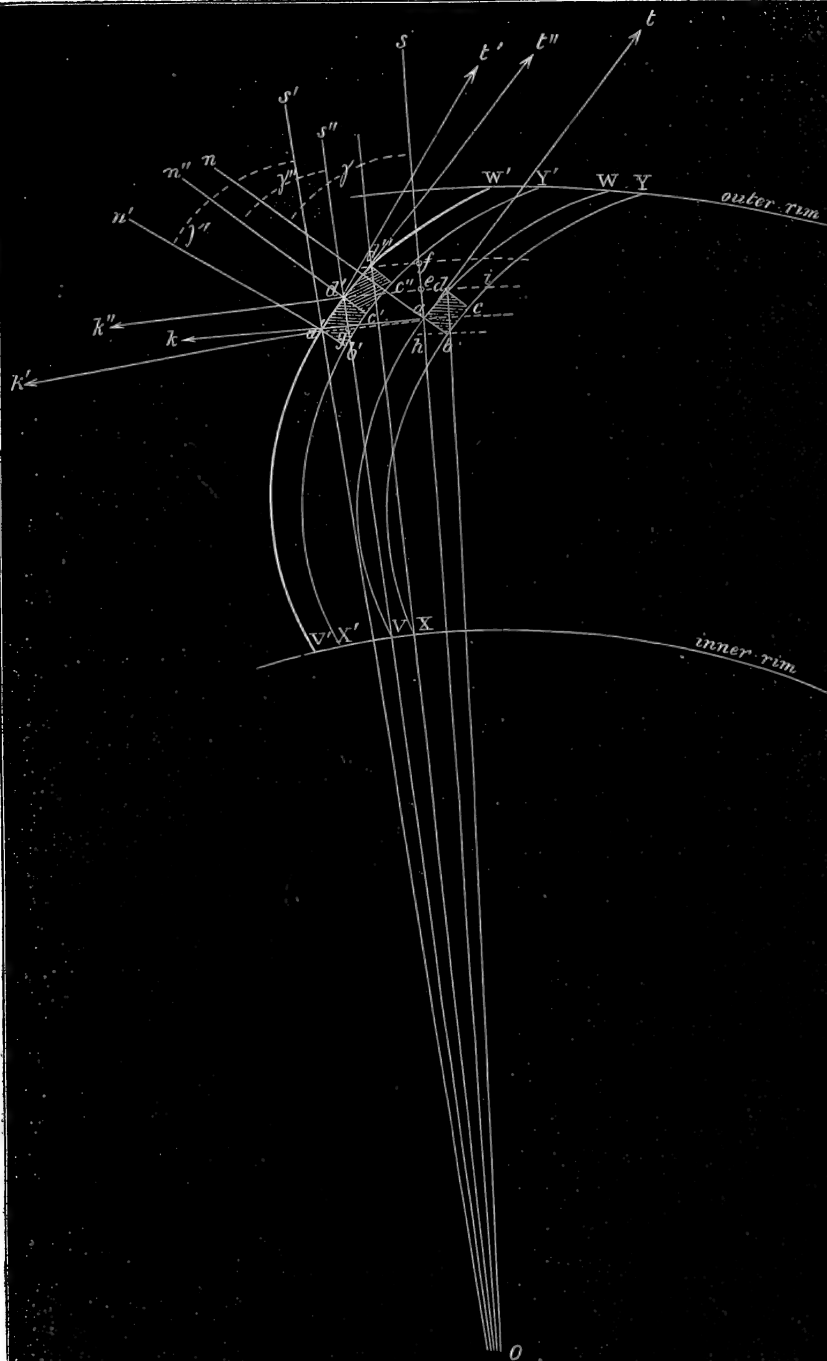
For the element of time, dt, we will take the time occupied by one of the liquid prisms in passing along the vane through a distance equal to its own length, or outwardly a distance $d\rho$, thus making t a function of ρ, the latter being considered the independent variable. We have

$$\frac{dt}{d\rho} d\rho = \frac{d\rho}{\dfrac{d\rho}{dt}} = \frac{d\rho}{v \sin \gamma}.$$

In the figure let aa' be the distance passed through by the turbine in the time dt, then

$$aa' = \omega\rho dt = \omega\rho \frac{dt}{d\rho} d\rho.$$

The mass m will have two motions; one along the vane, the other with the wheel perpendicular to the radius. By changing its position successively in each of these directions, both its velocity with the wheel and its velocity along the vane may suffer changes both in *amount* and *direction*, and will give rise to the following eight possible reactions :—

I. By moving from a to a' in the arc of a circle.

  1. $\omega\rho$ may be increased or diminished ;
  2. $\omega\rho$ may be changed in direction ;
  3. v may be increased or diminished ;
  4. v may be changed in direction.

II. By moving from a' to d' along the vane.

  5. $\omega\rho$ may be increased or diminished ;
  6. $\omega\rho$ may be changed in direction ;
  7. v may be increased or diminished ;
  8. v may be changed in direction.

By the conditions imposed, 1 and 3 are zero. For the others we have :—

No. 2. By moving from a to a', the velocity $\omega\rho$ is changed in direction from ak to $a'k'$ in the time dt. The momentum is $m\omega\rho$, and the rate of angular change is

$$\frac{kak'}{dt} = \frac{\omega dt}{dt} = \omega,$$

and hence the reaction will be $m\omega^2\rho$ in a direction radially outwards. This is the *centrifugal force* as designated by most writers. Resolving into two components, we have

$$m\omega^2 \rho \sin \gamma \text{ along the vane,}$$

$$m\omega^2\rho \cos \gamma \text{ normal to the vane.}$$

No. 4. In moving from a to a', the velocity along the vane, v, is changed in direction from at to $a't'$ at the rate ω as

in No. 3. The momentum is mv, and the force will be $mv\omega$, which acts in the direction na, and being resolved gives

$$0 \text{ along the vane,}$$
$$-mv\omega \text{ normal to the vane.}$$

No. 5. In passing from a' to d' the *increase* of $\omega\rho$ will be $\omega d\rho$ in a time dt, and the reaction will be $m\omega\dfrac{d\rho}{dt}$, in a direction tangential to the motion of the wheel but backwards, and its components will be

$$m\omega\frac{d\rho}{dt}\cos\gamma \text{ along the vane,}$$
$$-m\omega\frac{d\rho}{dt}\sin\gamma \text{ normal to the vane.}$$

No. 6. In passing from a' to d', $\omega\rho$ will be changed in direction by the angle $a'Od' = \dfrac{a'g}{\rho} = \dfrac{d\rho\cot\gamma}{\rho}$, and the rate of angular change will be

$$\frac{\cot\gamma}{\rho}\cdot\frac{d\rho}{dt},$$

and the momentum will be

$$m\omega\rho ;$$

hence the reaction will be

$$m\omega\cot\gamma\frac{d\rho}{dt},$$

which acts radially inward, and its components are

$$-m\omega\cos\gamma\frac{d\rho}{dt} \text{ along the vane,}$$
$$-m\omega\cot\gamma\cos\gamma\frac{d\rho}{dt} \text{ normal to the vane.}$$

No. 7. By moving from a' to d', v will be increased by an amount $\dfrac{dv}{d\rho}d\rho$, in the time dt, and the reaction will be $m\dfrac{dv}{d\rho}\cdot\dfrac{d\rho}{dt}$, which acts backward along the vane, and its components are

$$-m\frac{dv}{d\rho}\cdot\frac{d\rho}{dt} \text{ along the vane,}$$
$$0 \text{ normal to the vane.}$$

No. 8. In passing from a' to d', v is changed in direction by two amounts :—1st, the angle γ changes an amount $-\dfrac{d\gamma}{d\rho}d\rho$, and 2nd, the radius changes in direction an amount

$\dfrac{dp \cot \gamma}{\rho}$, as in No. 6 ; hence the total change will be the sum of these, and the *rate* of change will be the sum divided by dt, which result multiplied by the momentum, mv, will give the reaction, the components of which will be

0 along the vane,

$$mv\left[\frac{\cot \gamma}{\rho} \cdot \frac{dp}{dt} - \frac{d\gamma}{dp} \cdot \frac{dp}{dt}\right] \text{ normal to the vane.}$$

This completes the reactions. Next consider the *pressure* in the wheel. The *intensity* of the pressure on the two sides ab and cd differs by an amount $dp = \dfrac{dp}{d\rho} d\rho$. The area of the face is $dc \times x = x\rho d\theta \sin \gamma$, and the force due to the difference of pressures will be

$$x\rho d\theta \sin \gamma \frac{dp}{d\rho} d\rho.$$

If dp is positive, which will be the case when the pressure on dc exceeds that on ab, the force acts backwards, and the preceding expression will be *minus* along the vane.

In regard to the pressure normal to the vane, if a uniform pressure p existed from one end of the vane VW to the other, the resultant effect would be zero, since the pressure in one direction on VW would equal the opposite pressure on XY. If, however, the pressure at a exceeds that at d by an amount $-dp$, since Va is longer than Xb, the pressure on Va, due to this $-dp$, will exceed that on Xb by an amount

$$-dp \cdot x \times ah = -dp \cdot x \cdot \rho d\theta \cos \gamma = -x\rho \cos \gamma \, d\theta \frac{dp}{d\rho} d\rho.$$

Collecting these several reactions, we have

Normal to the vane.	Along the vane.
(2) $+ m\omega^2 \rho \cos \gamma.$	$+ m\omega^2 \rho \sin \gamma.$
(4) $- m\omega v.$	0.
(5) $- m\omega \sin \gamma \dfrac{d\rho}{dt}.$	$+ m\omega \cos \gamma \dfrac{d\rho}{dt}.$
(6) $- m\omega \cot \gamma \cos \gamma \dfrac{d\rho}{dt}.$	$- m\omega \cos \gamma \dfrac{d\rho}{dt}.$
(7) 0.	$- m\dfrac{d\rho}{dt} \cdot \dfrac{dv}{d\rho}.$
(8) $+ mv\left[\dfrac{\cot \gamma}{\rho} \cdot \dfrac{dp}{dt} - \dfrac{d\gamma}{dp} \cdot \dfrac{dp}{dt}\right].$	0.
(9) $- x\rho \cos \gamma \dfrac{dp}{d\rho} d\rho d\theta.$	$- x\rho \sin \gamma \dfrac{dp}{d\rho} d\rho d\theta.$

The sum of the quantities in the second column will be zero ; hence

$$m\omega^2 \rho \sin\gamma - m\frac{d\rho}{dt}\cdot\frac{dv}{d\rho} - x\rho \sin\gamma \frac{dp}{d\rho}\, d\rho d\theta = 0. \quad \cdot \quad (1)$$

Substituting

$$\frac{d\rho}{dt} = v\sin\gamma, \text{ and } x\rho d\theta d\rho = \frac{m}{\delta},$$

and dividing by $m\sin\gamma$, we have

$$\omega^2\rho d\rho - \frac{1}{\delta}\, dp = vdv. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

Integrating,

$$\left[\tfrac{1}{2}\omega^2\rho - \frac{p}{\delta}\right]_{\text{limit}}^{\text{limit}} = \left[\tfrac{1}{2}v^2\right]_{\text{limit}}^{\text{limit}}. \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

The sum of the quantities in the first column gives the pressure normal to the vane, which multiplied by $\rho \sin\gamma$ gives the moment. This done, and substituting as before, we have

$$d^2\mathrm{M} = mv\left[\omega\rho\Big(\frac{\rho}{v}\omega\cos\gamma - 2\Big) - \rho v\sin\gamma\frac{d\gamma}{d\rho} + v\cos\gamma - \rho\frac{\cos\gamma}{v\delta}\frac{dp}{d\rho}\right]\sin\gamma.$$

Putting $mv\sin\gamma = \dfrac{\delta Q}{2\pi}d\theta d\rho$, where Q is the quantity of water flowing through the wheel per second, and integrating in reference to θ between 0 and 2π, we have

$$d\mathrm{M} = \delta Q\left[\omega\rho\Big(\frac{\rho}{v}\omega\cos\gamma - 2\Big) - \rho v\sin\gamma\frac{d\gamma}{d\rho} + v\cos\gamma - \rho\frac{\cos\gamma}{v\delta}\frac{dp}{d\rho}\right]d\rho.$$

Multiplying (2) by $\dfrac{\rho}{v}\cos\gamma$, we have

$$\frac{\omega^2\rho^2}{v}\cos\gamma\, d\rho - \frac{\rho\cos\gamma}{v\delta}\frac{dp}{d\rho}\, d\rho = \rho\cos\gamma\frac{dv}{d\rho}\, d\rho,$$

which, substituted above, gives

$$d\mathrm{M} = \delta Q\left[-2\omega\rho d\rho + \rho\cos\gamma\frac{dv}{d\rho}\, d\rho + v\cos\gamma d\rho - \rho v\sin\gamma\frac{d\gamma}{d\rho}\, d\rho\right], \quad (4)$$

and integrating,

$$\mathrm{M} = \delta Q[-\omega\rho^2 + \rho v\cos\gamma]$$
$$= -\delta Q\rho[\omega\rho - v\cos\gamma]_{\text{lim.}}^{\text{lim.}} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

But $\omega\rho - v\cos\gamma$ is the circumferential velocity in space of

the water at any point, and $\delta Q\rho[\omega\rho - v\cos\gamma]$ is the moment of the momentum ; hence, integrating between limits for the inner and outer rims, *the moment exerted by the water on the wheel equals the difference in its moment of momentum on entering and leaving the wheel.* Thus we have deduced an expression which some writers have made the basis of their investigations.

Let the values of the variables at the entrance of the wheel be

$$\rho_1, \ \gamma_1, \ v_1, \ p_1,$$

and at exit be

$$\rho_2, \ \gamma_2, \ v_2, \ p_2.$$

Then equations (3) and (5) become

$$\tfrac{1}{2}\omega^2(\rho_1{}^2 - \rho_2{}^2) - \frac{p_1 - p_2}{\delta} = \tfrac{1}{2}(v_1{}^2 - v_2{}^2). \quad . \quad . \quad . \quad (6)$$

$$M = \delta Q[\omega(\rho_1{}^2 - \rho_2{}^2) - \rho_1 v_1\cos\gamma_1 + \rho_2 v_2\cos\gamma_2]. \quad (7)$$

$$\therefore U = M\omega = \delta Q\omega[\omega(\rho_1{}^2 - \rho_2{}^2) - \rho_1 v_1\cos\gamma_1 + \rho_2 v_2\cos\gamma_2]. \quad (8)$$

Equation (8) gives the work per second in terms of the known quantities δ, ω, ρ_1, γ_1, ρ_2, γ_2, and the three quantities Q, v_1, v_2 as yet unknown. These three quantities are, however, connected by the condition that the quantity of water flowing through all the sections radially is constant. Calling a_1 the entire area of all the orifices at the entrance of the wheel ($= 2\pi\rho_1 x_1$), and a_2 those at exit ($= 2\pi\rho_2 x_2$), we have

$$Q = a_1 v_1\sin\gamma_1 = a_2 v_2\sin\gamma_2, \quad . \quad . \quad . \quad (9)$$

which reduces this number of unknown quantities to one.

Equation (6) is the equation of the motion of the water in the wheel. Besides the velocities v_1 and v_2, it contains p_1 and p_2.

Let

$p_a = $ the atmospheric pressure,

$h \ = $ the mean depth of the wheel below the surface of the tail-race,

$p_t = \delta g h = $ the pressure due to flooding in the tail-race ; then

$$p_2 = p_a + p_t.$$

The pressure p_1 where the water passes from the guide-plates into the wheel is unknown. Another condition is necessary, which may be found by considering the passage of the water from the guide-plates into the wheel. In fig. 2 let A C be the tangent to the guide-plate at its extremity, V the actual velocity of the water on leaving the guide-plate, $\omega\rho_1 = $ A D the velocity of the initial rim of the wheel ; then

will A B be the velocity of the stream relative to the wheel. Now if A B does not coincide with the tangent to the vane at A, the stream cannot suddenly be made to change its direction into that of the vane, or float ; and the water, by cushioning in the angles, will make its own angles, as roughly shown in fig. 3.
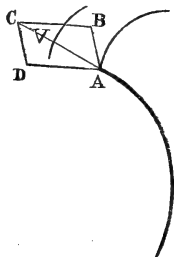
Fig. 2.   Fig. 3.
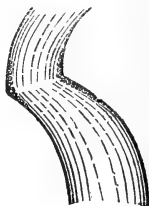
It is impossible, either practically or theoretically, to determine the new angles, and probably they are not constant ; neither is it possible to determine the loss of energy due to eddying ; we therefore make the hypothesis that the final direction of the guide-plates, the initial direction of the vanes, the angular velocity of the wheel, and the velocity of the flow, are so related that the water on leaving the guide-plates shall coincide in direction with the initial elements of the vanes. Any three of the four quantities above mentioned being fixed, the fourth becomes known by this relation. We will leave the angle of the guide-plates to be determined later.

Let

V = the actual velocity of the water on leaving the guide-plates.
v_1 = the velocity relative to the vane, as before.

Then V must be the resultant of $\omega\rho_1$ and v_1, and we have

Fig. 4.

$$V^2 = v_1^2 + \omega^2\rho_1^2 - 2v_1\omega\rho_1 \cos\gamma_1. \quad (10)$$

From Bernouilli's theorem we have for the flow of water in the head-race the equation

$$\frac{p_a}{\delta g} + \mathfrak{h} = \frac{p_1}{\delta g} + \frac{V^2}{2g}, \quad . \quad (11)$$

$\mathfrak{h}$ being the mean height of the surface of the water in the reservoir above the wheel. We thus have two equations, (10) and (11), introducing one new unknown quantity.
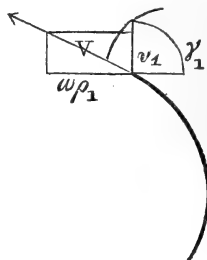
Eliminating V in (10) and (11), we have

$$v_1{}^2 + \omega^2\rho_1{}^2 - 2v_1\omega\rho_1\cos\gamma_1 = 2g\mathfrak{h} - \frac{2p_1}{\delta} + \frac{2p_a}{\delta}. \quad (12)$$

Substituting in equation (6), $p_2 = p_a + g\delta h$, we have

$$\omega^2\rho_1{}^2 - \omega^2\rho_2{}^2 - \frac{2p_1}{\delta} + \frac{2p_a}{\delta} + 2gh = v_1{}^2 - v_2{}^2. \quad (13)$$

Adding (12) and (13), we have

$$2\omega^2\rho_1{}^2 - \omega^2\rho_2{}^2 = 2g(\mathfrak{h} - h) + 2v_1\omega\rho_1\cos\gamma_1 - v_2{}^2. \quad (14)$$

From (9) and (14), H being substituted for $(\mathfrak{h} - h)$, we find

$$v_1 = \frac{a_2{}^2\sin^2\gamma_2}{a_1{}^2\sin^2\gamma_1}\,\omega\rho_1\cos\gamma_1$$
$$+\sqrt{\frac{a_2{}^2\sin^2\gamma_2(\omega^2\rho_2{}^2 - 2\omega^2\rho_1{}^2 + 2gH)}{a_1{}^2\sin^2\gamma_1} + \frac{a_2{}^4\sin^4\gamma_2}{a_1{}^4\sin^4\gamma_1}\,\omega^2\rho_1{}^2\cos^2\gamma_1}. \quad (15)$$

$$v_2 = \omega\rho_1\frac{a_2\sin\gamma_2}{a_1\sin\gamma_1}\cos\gamma_1$$
$$+\sqrt{\omega^2\rho_2{}^2 - 2\omega^2\rho_1{}^2 + 2gH + \frac{a_2{}^2\sin^2\gamma_2}{a_1{}^2\sin^2\gamma_1}\,\omega^2\rho_1{}^2\cos^2\gamma_1}. \quad\quad (15a)$$

$$Q = \omega\rho_1\frac{a_2{}^2\sin^2\gamma_2}{a_1\sin\gamma_1}\cos\gamma_1$$
$$+ a_2\sin\gamma_2\sqrt{\omega^2\rho_2{}^2 - 2\omega^2\rho_1{}^2 + 2gH + \frac{a_2{}^2\sin^2\gamma_2}{a_1{}^2\sin^2\gamma_1}\,\omega^2\rho_1{}^2\cos^2\gamma_1}. \quad (16)$$

The efficiency will be, from equation (8),

$$E = \frac{U}{g\delta QH} = \frac{\omega}{gH}\left[\omega^2\rho_1{}^2 - \omega^2\rho_2{}^2 - \rho_1v_1\cos\gamma_1 + \rho_2v_2\cos\gamma_2\right], \quad (17)$$

which, substituting the values of v_1 and v_2, gives

$$E = \frac{1}{gH}\left\{\omega^2\rho_1{}^2 - \omega^2\rho_2{}^2 + \omega\left[\rho_2\cos\gamma_2 - \frac{a_2\sin\gamma_2}{a_1\sin\gamma_1}\rho_1\cos\gamma_1\right]\right.$$
$$\left.\frac{a_2\sin\gamma_2}{a_1\sin\gamma_1}\,\omega\rho_1\cos\gamma_1 + \sqrt{\omega^2\rho_2{}^2 - 2\omega^2\rho_1{}^2 + 2gH + \frac{a_2{}^2\sin^2\gamma_2}{a_1{}^2\sin^2\gamma_1}\omega^2\rho_1{}^2\cos^2\gamma_1}\right]\right\}. \quad (18)$$

To find the angular velocity that will give a maximum efficiency, make $\dfrac{dE}{d\omega} = 0$, in (18).

For brevity make

$$n = \rho_2 \cos \gamma_2 - \frac{a_2 \sin \gamma_2}{a_1 \sin \gamma_1} \rho_1 \cos \gamma_1, \quad . \quad . \quad . \quad (19)$$

$$b = \frac{a_2 \sin \gamma_2}{a_1 \sin \gamma_1} \rho_1 \cos \gamma_1, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$$l^2 = \rho_2{}^2 - 2\rho_1{}^2 + \frac{a_2{}^2 \sin^2 \gamma_2}{a_1{}^2 \sin^2 \gamma_1} \rho_1{}^2 \cos^2 \gamma_1, \quad . \quad . \quad (21)$$

$$s^2 = N \frac{a_2 \sin \gamma_2}{a_1 \sin \gamma_1} \rho_1 \cos \gamma_1 + \rho_1{}^2 - \rho_2{}^2, \quad . \quad . \quad . \quad (22)$$

then $\dfrac{d\mathrm{E}}{d\omega} = 0$ will give

$$-\omega s^2 \sqrt{\omega^2 l^2 + 2g\mathrm{H}} = n^2 \omega l^2 + ng\mathrm{H}, \quad . \quad . \quad . \quad (23)$$

$$\omega^2 = \frac{g\mathrm{H}}{l^2} \left[\frac{s^2 - \overline{s^4 - n \sqrt{{}^2 l^2}}}{\sqrt{s^2 - n^2 l^2}} \right] ; \quad . \quad . \quad . \quad (24)$$

and this substituted in (18) will give the maximum efficiency for any turbine, and in (16) will give the quantity of water discharged. It would be simpler to find ω numerically for any particular case before making the substitution.

We will now use these equations to show some errors made by Rankine and Weisbach. Rankine, in his 'Steam-Engine and other Prime Movers,' discusses a special wheel of the Fourneyron type, in which he assumes

$$a_2 = a_1 \text{ and } \gamma_1 = 90°.$$

These in equation (15) give

$$v_1 = \sin \gamma_2 \sqrt{\omega^2 \rho_2{}^2 - 2\omega^2 \rho_1{}^2 + 2g\mathrm{H}}. \quad . \quad . \quad (25)$$

This velocity is radial along the vane, and is called by Rankine the "velocity of flow." The tangential component of the actual velocity must, in this case, be $\omega\rho_1$, and this, by Rankine, is called the velocity of whirl (v) ; and his value of v on page 196, equation (9) of the 'Steam-Engine' is not only incorrect but meaningless, even for the turbine he is considering.

From equation (15a) we also have

$$v_2 = \sqrt{\omega^2 \rho_2{}^2 - 2\omega^2 \rho_1{}^2 + 2g\mathrm{H}}. \quad . \quad . \quad (26)$$

The expression for the efficiency becomes

$$\mathrm{E} = \frac{\omega}{g\mathrm{H}} \left[\omega\rho_1{}^2 - \omega\rho_2{}^2 + \rho_2 \cos \gamma_2 \sqrt{\omega^2 \rho_2{}^2 - 2\omega^2 \rho_1{}^2 + 2g\mathrm{H}} \right]. \quad (27)$$

This expression differs entirely from Rankine's equation
(10), page 196. But running this wheel at the same speed
as Rankine does his, Art. 175, that is, making the final
velocity of whirl zero, we shall have

$$v_2 \cos \gamma_2 = \omega \rho_2,$$

and equation (26) becomes

$$\omega \rho_2 = \cos \gamma_2 \sqrt{\omega^2 \rho_2{}^2 - 2\omega^2 \rho_1{}^2 + 2g\mathrm{H}} \; ;$$

from which we find

$$\omega = \sqrt{\frac{2g\mathrm{H}}{2\rho_1{}^2 + \rho_2{}^2 \tan^2 \gamma_2}} \; ; \quad \cdot \quad \cdot \quad \cdot \quad (28)$$

$$\therefore \; \omega \rho_1 = \sqrt{\frac{2g\mathrm{H}}{2 + \dfrac{\rho_2{}^2}{\rho_1{}^2} \tan^2 \gamma_2}},$$

which is the same as Rankine's equation (3), Art. 175. Sub-
stituting these in (27), we have

$$\mathrm{E} = \frac{2\rho_1{}^2}{2\rho_2{}^2 + \rho_2{}^2 \tan^2 \gamma_2},$$

which is Rankine's equation (4), Art. 175.

We thus see that Rankine's equations not only do not fit
any wheel except the one he is considering, but they apply to
that only at one particular speed. These conclusions agree
with those in an article on Turbines, by Professor Wood, in
the Journal of the Franklin Institute for June 1884.

In regard to the speed for maximum efficiency, Rankine,
in the 'Steam-Engine,' Art. 173, says, "In order that the
water may work to the best advantage, it should leave the
wheel without whirling motion, for which purpose the velocity
of whirl relative to the wheel should be equal and contrary to
that of the second circumference of the wheel." Plausible
as this appears, it is true only for special cases even for his
wheel. Also Weisbach makes the erroneous statement that
the velocity of the second rim of the wheel should equal the
relative velocity of discharge. Thus in 'The Mechanics of
Engineering and of Machinery,' vol. ii. page 400 (Wiley and
Sons) he says (substituting my notation for his)

$$w = \sqrt{\omega^2 \rho_2{}^2 + v_2{}^2 - 2v_2 \omega \rho_2 \cos \gamma_2} = \sqrt{(\omega \rho_2 - v_1)^2 + 4\omega \rho_2 v_2 \sin^2 \frac{\gamma_2}{2}},$$

in regard to which he states that for w a minimum $\omega\rho_2$ must equal v_1, which is not *generally* true, and is true only when $\gamma_2=0$, or when $v_2\omega\rho_2$ and $(\omega\rho_2-v_2)^2$ happen to be a minimum together. The value of ω in equation (24) will not in general satisfy Rankine's condition

$$\omega\rho_2=v_2\cos\gamma_2, \quad . \quad . \quad . \quad . \quad . \quad (29)$$

nor Weisbach's

$$\omega\rho_2=v_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (29a)$$

Substituting in equation (24) Rankine's condition $\gamma_1=90°$, and making $r=\dfrac{\rho_1}{\rho_2}$, we find

$$\omega^2\rho_2{}^2=\frac{g\mathrm{H}}{1-2r^2}\left[\frac{1-r^2-\sqrt{(1-r^2)^2-\cos^2\gamma_2(1-2r^2)}}{\sqrt{(1-r^2)^2-\cos^2\gamma_2(1-2r^2)}}\right]. \quad (30)$$

Substituting this in (15a) gives

$$v^2\cos\gamma_2=\omega\rho_2\left[1-r^2+\sqrt{(1-r^2)^2-\cos^2\gamma_2(1-2r^2)}\right]. \quad (31)$$

This satisfies equation (29) only when $r^2=\frac{1}{2}$, and (29a) only when $\gamma_2=0$ or $r=1$. The latter condition is that of a parallel flow wheel, or of an infinitely narrow wheel, in which case we have

$$\omega\rho_2=v_2=\sqrt{g\mathrm{H}}.$$

These in (17) give for the efficiency

$$\mathrm{E}=\cos\gamma_2,$$

which always exceeds the value given by Rankine, when $r=1$,

$$\mathrm{E}=\frac{2\cos^2\gamma_2}{1+\cos^2\gamma_2},$$

except when $\gamma_2=0$, when both become unity, but the work done will be zero.

The pressure in the wheel may be found by integrating equation (3) between initial and general limits, and eliminating p_1 and v by means of equations (12) and (9), giving

$$\frac{2p}{\delta}=\omega^2\rho^2-2\omega^2\rho_1{}^2+\frac{2p_a}{\delta}+2g\mathfrak{y}+2v_1\omega\rho_1\cos\gamma_1-v_1{}^2\frac{a_1{}^2\sin^2\gamma_1}{a^2\sin^2\gamma}, \quad (32)$$

which may be discussed for the various conditions to which the wheel is subjected.

XXXVIII. *The Expansion of Mercury between* 0° C. *and* −39° C. *By* Professors W. E. AYRTON, *F.R.S., and* JOHN PERRY, *F.R.S.*[*]

A T a meeting of the Physical Society in November 1885, Mr. G. Whipple gave the Society the results of the examination of thermometers down to the melting-point of mercury. There was, however, no evidence as to whether the contraction of the mercury was uniform, as the thermometers were only compared with mercurial ones, and as, in addition, we were not able to find the results of any experiments made on the expansion of mercury between 0° and −39° C., its temperature of solidification, we thought it desirable to make a series of comparisons of a mercury-thermometer, the stem of which had been accurately subdivided into equal volumes, with an air-thermometer, both immersed in a bath of frozen mercury which was allowed to gradually become warm. For this purpose we borrowed a mercury-thermometer from Mr. Whipple, which he was so kind as to lend us, and one of our assistants (Mr. Mather) constructed a very simple form of constant-volume air-thermometer, shown in the diagram.
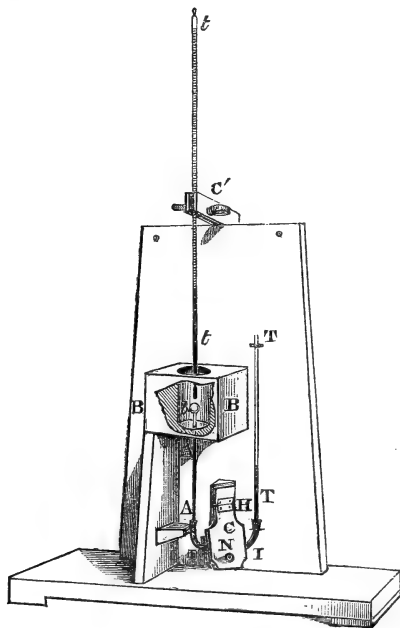
B B is a wooden box, at the bottom of which is a hole closed

by an india-rubber stopper through which passes tightly the glass stem of an air-thermometer, A A. The bottom of the air-thermometer is attached by a piece of india-rubber tubing, I I, to a vertical glass tube, T T; and the thermometer being filled with dry air, some mercury is introduced into the tube so as to stand at about the same height in the two limbs when the air in the bulb *b* is at atmospheric pressure. The height of the level of the mercury can be varied by turning the nut, N, which causes the clamp, C, turning on the hinge H, to squeeze the india-rubber tube more or less tightly, and so to alter its internal capacity ; and in this way the level of the mercury in the left-hand tube can be kept quite fixed, and therefore the volume of air in the bulb *b* quite constant, while its temperature is altered, the corresponding pressure being of course measured by the difference between the levels of the mercury columns in the two limbs.

To perform the experiment, the box B B was first filled with mercury and frozen by stirring carbonic-acid snow and ether up with it ; and when sufficient of the mercury was frozen, the bulb of the mercurial thermometer, *t t*, was introduced into the pasty mercury, and the thermometer fixed in position by means of the clamp, C′. The nut N was now turned by one observer until the level of the mercury in the left-hand limb came opposite a *fixed* mark on the tube which is only just below the bottom of the box, when the height of the mercury in the right-hand tube was read by a cathetometer made by the Cambridge Instrument Company, and the position of the mercury in the mercury-thermometer was read by a third observer. In this way several series of simultaneous observations were taken, during the course of some weeks, of the pressure to which the air in the air-thermometer had to be subjected to keep its volume constant, as the mercury in the box B B varied in temperature from about −39° C. to 0° C. Plotting these results, it was found that they lay in so nearly a straight line that we may conclude that mercury expands regularly below 0° C. as it is known to do above 0° C. ; and that there is no critical point for mercury, as there is for water, above the freezing-point.

When the mercury freezes it contracts still further, as may be seen from the following extract from page 6 of the second volume of Nordenskiöld's 'Voyage of the Vega,' which Mr. Whipple has kindly looked up for us :—

" When mercury freezes in a common thermometer, it contracts so much that the column of mercury suddenly sinks in the tube, or, if it is short, goes wholly into the ball. The position of the column is therefore no measure of the actual degree of cold when the freezing takes place."

We have to express our thanks to Messrs. Chatterton, Humphrey, and Martin, three of the students of the Central Institution, for assistance rendered in the carrying out of this experiment.

XXXIX. *On the Expansion produced by Amalgamation.   By* Professors W. E. AYRTON, *F.R.S., and* JOHN PERRY, *F.R.S.*[*]

O N amalgamating the edge of a brass bar, nearly three quarters of an inch thick and about a foot long, for the purpose of enabling the edge to make good electric contact with a plate, we were surprised to find that the bar rapidly curved, the amalgamated edge becoming convex, exactly as happens when one side of a piece of paper is wetted.   On hammering the bar to straighten it, the curvature became instead greater.   Seeing that to bend a short brass bar more than half an inch in thickness to the extent produced by the amalgamation of the edge requires the exertion of very considerable stresses, it follows that very great forces must be produced by amalgamation.

We think it possible that this bending by amalgamation may be an important cause in the production of the Japanese " magic mirrors," the reflecting surface of which is polished with a mercury amalgam.   Japanese mirrors are made of bronze and have a raised pattern cast on their backs ; and although the eye can detect no trace of the pattern on looking at the polished reflecting surface, yet when certain of these mirrors are used to reflect a divergent beam on to a screen, the pattern at the back can be seen as a bright image on a dark ground.   In a paper communicated, some years ago, to the Royal Society, we showed that this peculiar effect arose from the fact that, while the reflecting surface was generally convex, the portions corresponding with the pattern or thicker parts were less convex (that is, more concave) than the rest ; and this conclusion we verified by finding that when a convergent, instead of a divergent, beam of light was allowed to fall on the mirror the image on the screen was reversed ; that is, the pattern was seen as a dark image on a bright ground.

This inequality of curvature we considered at that time was due partly to the pressure of the " distorting-rod " used to make the surface convex, and partly to the pressure exercised on the subsequent polishing ; but we now think that, in addition, the action of the mercury-amalgam employed by the polisher may assist in making the thin portions of the mirror more concave than the thicker.

[*] Communicated by the Physical Society : read March 27, 1886.

XL. *Note on the Annual Precession calculated on the Hypo-thesis of the Earth's Solidity.* By HENRY HENNESSY, *F.R.S.,* *Professor of Applied Mathematics in the Royal College of Science for Ireland* [*].

IN discussing the influence of the internal structure of the Earth upon precession, it has been frequently assumed that with the ellipticity $\frac{1}{300}$ the annual precession of a homo-geneous solid shell or completely solid spheroid would be 57″. This was the result of Mr. Hopkins's calculations; and the difference, amounting to between six and seven seconds, between it and the observed value, formed the basis of all his con-clusions relative to the Earth's internal condition. Hitherto I have not seen any reason for doubting the above numerical result; but on looking more closely into the question, it appears probable that we must reduce the precession for the hypo-thetical solid spheroid to about 55″. If the Earth were a spheroid perfectly rigid, the amount of precession can be calculated from formulæ given in Airy's 'Tracts,' Pratt's 'Mechanical Philosophy,' Pontécoulant's *Théorie Analytique du Système du Monde,* or Resal's *Traité de Mécanique Céleste.* In the two latter works, Poisson's memoir on the rotation of the Earth about its centre of gravity is very closely followed, and the formulæ are those which I have generally employed. From these writings we find

$$P_1 = \frac{3m^2(2C-A-B)}{4n} \cdot \frac{C}{C} (1+\gamma) \cos I ;$$

where I is the inclination of the equator to the ecliptic, γ the ratio of the Moon's action on the Earth compared to that of the Sun, m the Earth's mean motion around the Sun, $\frac{m}{n}$ the ratio of this mean motion to the Earth's rotation, and A, B, C the three principal movements of inertia of the Earth. When the Earth is supposed to be a spheroid of revolution A=B, and the above becomes

(1) $P = \frac{3}{2} \frac{m^2}{n} \frac{C-A}{C} \cdot (1+\gamma) \cos I.$

Pratt gives the formula

(2) $P = \frac{3}{2} \frac{n^1}{n} \left(\frac{C-A}{A} \right) \left\{ 1 + \frac{n^2}{n^1} \frac{1 - \frac{3}{2} \sin^2 i}{1+\gamma} \right\} 180° ;$

where i is the inclination of the Moon's orbit to the ecliptic, γ the ratio of the Earth's mass to that of the Moon.

In all these formulæ, or in any others by which the precession can be calculated, the Moon's mass enters directly or indirectly. When Mr. Hopkins made his calculation, more than forty years ago, he appears to have taken the value of the Moon's mass and all his other numerical data from the early editions of Airy's 'Tracts.' He uses 366·26 for the Earth's period, 27·32 for the Moon's. He makes $I = 23^\circ\ 28'$, $i = 5^\circ\ 8'\ 50''$, and the Moon's mass $= \dfrac{1}{70}$ of the Earth's mass. All of these values require revision; and it may be remarked that Sir George Airy has more recently expressed the opinion that $\dfrac{1}{80}$ may be taken as the value of the Moon's mass. (Monthly Notices of the Royal Astronomical Society, December 1878, p. 140). On this question, I may be permitted to remark that there are three different phenomena from which the Moon's mass has been determined:—1, the perturbations of the Earth's motion in its orbit around the Sun by the action of the Moon ; 2, the Tides ; and 3, the Nutation of the Earth's axis. The largest mass, or $\dfrac{1}{70}$ nearly, has been obtained from the first, and the smallest from Nutation. But the values obtained from Nutation are not very accordant, and, moreover, the close connection between Nutation and Precession makes it a doubtful matter to calculate the amount of one from a quantity depending on the other. The Moon's mass obtained from the Tides is that which has been employed by Laplace, Poisson, and other mathematicians as the most probable. It appears that a recent discussion of the Tides in the United States, made by Mr. Ferrel, has given the same value as that found by Laplace. This circumstance, as well as the fact that the value so obtained lies between the values found by the other methods, give us reason to place much confidence in the result. If we call P_1 the precession for a homogeneous spheroid whose ellipticity is E, then from (1)

$$P_1 = \frac{3}{2}\frac{m^2}{n}E\,(1+\gamma)\cos I.$$

If we take the value of the Moon's mass given by the tides, or rather the ratio of the Moon's action to that of the Sun thus given, we shall use the value of γ employed by Poisson, Pontécoulant, and Resal: if we also employ for E the value which Colonel Clarke shows good grounds for deeming the

most probable *, that is $\frac{1}{293\cdot46}$ instead of $\frac{1}{300}$, or even smaller fractions hitherto accepted, I find that P_1 becomes $56''\cdot05$. By Pratt's formula and the numerical values he employs, except for E, I find

$$P_1 = 54''\cdot879.$$

If we take $\frac{1}{80}$ for the Moon's mass in Poisson's formula, γ becomes $2\cdot2062$, and

$$P_1 = 53''\cdot574.$$

If we change γ to 80 in Pratt's formula with

$$E = \frac{1}{293\cdot46}, \quad P_1 = 52''\cdot95.$$

The value for the observed precession now generally admitted is $50''\cdot37$. It is therefore manifest that the difference between this and the precession of a homogeneous equi-elliptic spheroid cannot be admitted to be as great as Mr. Hopkins has declared it to be. From the values of P_1 which I have calculated we should have

$$P_1 - P = 5''\cdot68 \text{ and } 4''\cdot507,$$

with the Moon's mass $= \frac{1}{75}$;

$$P_1 - P = \frac{3''\cdot204}{3''\cdot617}, \text{ and } 2''\cdot58,$$

if we take the Moon's mass $= \frac{1}{80}$.

On calculating P with the Moon's mass $= \frac{1}{80}$, Sun's mass 354936, γ is $2\cdot25395$. If we take for I its value in 1852, or $23° \; 27' \; 32''$, and make

$$m = 359°\cdot9931, \quad \frac{m}{n} = \cdot0027303, \quad E = \frac{1}{293\cdot46},$$

the following calculations can be made :—

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* See Colonel Clarke's paper in the Philosophical Magazine for August 1878, where he maintains that recent geodesical results tend to increase the value of the Earth's ellipticity and to make the measured value approach to that obtained from pendulum observations.

$$\log m = 2\cdot 5562965,$$

$$\log (1+\gamma) = 0\cdot 5124109,$$

$$\log \cos I = 9\cdot 9625322,$$

$$\log \frac{m}{n} = \frac{-3+4362104}{\cdot 4674500},$$

$$\log \frac{3}{2}[60\times 60] = \frac{3\cdot 7323937}{4\cdot 1998437},$$

$$\log P_1 = \frac{2\cdot 4675489}{1\cdot 7322948} = \log 53''\cdot 988,$$

or $P_1 = 54''$ nearly, $P_1 - P = 3''\cdot 617.$

Consequently, instead of admitting Mr. Hopkins's result of $7''$ for the difference between the precession of a homogeneous spheroid with the Earth's ellipticity and the precession actually observed, we may affirm that this difference is probably not more than $4''$ or $5''$.

With the best values for the numerical elements the difference is, however, too well ascertained to be overlooked, and it leads to the conclusion that the Earth cannot consist of an entirely solid mass composed of equielliptic strata, and that it is therefore partly composed of a solid shell bounded by surfaces such as I have elsewhere indicated, with an interior mass of viscid liquid, such as is seen flowing from the volcanic openings of the shell, arranged in strata conforming to the laws of hydrostatics, or, in other words, with strata of equal density decreasing in ellipticity towards the Earth's centre.

NOTE.—The section shown in the engraving at p. 245 of the paper in the Number for September should be placed thus, with the longer axis parallel to the lines of the page.
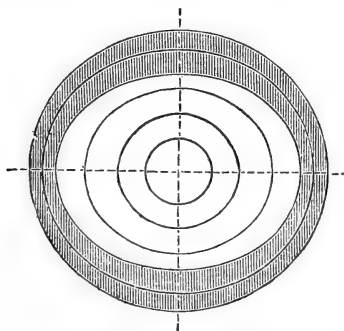
Erratum in same Paper.

Line 16 from top of p. 247, *for*

$$-\int\rho(x^2+y^2)\,dx\,dy\,dz \quad \text{read} \quad -\int\rho(z^2+y^2)\,dx\,dy\,dz.$$

XLI. *On the Self-induction of Wires.*—Part III.
By Oliver Heaviside[*].

THE subject of the decomposition of an arbitrary function into the sum of functions of special types has many fascinations. No student of mathematical physics, if he possess any soul at all, can fail to recognize the poetry that pervades this branch of mathematics. The great work of Fourier is full of it, although there only the mere fringe of the subject is reached. For that very reason, and because the solutions can be fully realized, the poetry is more plainly evident than in cases of greater complexity. Another remarkable thing to be observed is the way the principle of conservation of energy and its transfer, or the equation of activity, governs the whole subject, in dynamical applications, as regards the possibility of effecting certain expansions, the forms of the functions involved, the manner of effecting the expansions, and the possible nature of the " terminal conditions " which may be imposed.

Special proofs of the possibility of certain expansions are sometimes very vexatious. They are frequently long, complex, difficult to follow, unconvincing, and, after all, quite special ; whilst there are infinite numbers of functions equally deserving. Something of a quite general nature is clearly wanted, and simple in its generality, to cover the whole field. This will, I believe, be ultimately found in the principle of energy, at least as regards the functions of mathematical physics. But in the present place only a small part of the question will be touched upon, with special reference to the physical problem of the propagation of electromagnetic disturbances through a dielectric tube, bounded by conductors.

It will be, perhaps, in the recollection of some readers that Professor Sylvester, a few years since, in the course of his learned paper on the Bipotential, poked fun at Professor Maxwell for having, in his investigation of the conjugate properties possessed by complete spherical-surface harmonics, made use of Green's Theorem concerning the mutual energy of two electrified systems. He said (in effect, for the quotation is from memory) that one might as well prove the rule of three by the laws of hydrostatics, or something similar to that. In the second edition of his treatise, Prof. Maxwell made some remarks that appear to be meant for a reply to this ; to the effect that although names, involving physical ideas, are given to certain quantities, yet as the reasoning is purely mathematical, the physicist has a right to assist himself by the physical ideas.

[*] Communicated by the Author.

Certainly; but there is much more in it than that. For not only the conjugate properties of spherical harmonics, but those of all other functions of the fluctuating character, which present themselves in physical problems, including the infinitely undiscoverable, are involved in the principle of energy, and are most simply and immediately proved by it, and predicted beforehand. We may indeed get rid of the principle of energy, and treat the matter as a question of the properties of quadratic functions; a method which may commend itself to the pure mathematician. But by the use of the principle of energy, and assisted by the physical ideas involved, we are enabled to go straight to the mark at once, and avoid the unnecessary complexities connected with the use of the special functions in question, which may be so great as to wholly prevent the recognition of the properties which, through the principle of energy, are necessitated.

Considering only a dynamical system in which the forces of reaction are proportional to displacements, and the forces of resistance to velocities, there are three important quantities—the potential energy, the kinetic energy, and the dissipativity, say U, T, and Q, which are quadratic functions of the variables or their velocities. When there is no kinetic energy, the conjugate properties of normal systems are $U_{12}=0$ and $Q_{12}=0$; these standing for the mutual potential energy and the mutual dissipativity of a pair of normal systems. When there is no potential energy, we have $T_{12}=0$ and $Q_{12}=0$. When there is no dissipation of energy, $U_{12}=0$ and $T_{12}=0$. And in general, $U_{12}=T_{12}$, which covers all cases, and has two equivalents, $\frac{1}{2}Q_{12}+\dot{U}_{12}=0$, and $\frac{1}{2}Q_{12}+\dot{T}_{12}=0$; for, as the mutual potential and kinetic energies are equal, the mutual dissipativity is derived half from each.

Let the variables be $x_1, x_2, \ldots$, their velocities $v_1 = \dot{x}_1, \ldots$, and the equations of motion

$$\left.\begin{array}{l} F_1 = (A_{11} + B_{11}p + C_{11}p^2)x_1 + (A_{12} + B_{12}p + C_{12}p^2)x_2 + \ldots, \\ F_2 = (A_{21} + B_{21}p + C_{21}p^2)x_1 + (A_{22} + B_{22}p + C_{22}p^2)x_2 + \ldots, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad (88)$$

where $F_1, F_2, \ldots$, are impressed forces, and p stands for d/dt. Forming the equation of total activity we obtain

$$\Sigma Fv = Q + \dot{U} + \dot{T}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (89)$$

where

$$\left.\begin{array}{l} 2U = A_{11}x_1^2 + 2A_{12}x_1x_2 + A_{22}x_2^2 + \ldots, \\ Q = B_{11}v_1^2 + 2B_{12}v_1v_2 + B_{22}v_2^2 + \ldots, \\ 2T = C_{11}v_1^2 + 2C_{12}v_1v_2 + C_{22}v_2^2 + \ldots \end{array}\right\} \quad \cdot \quad (90)$$

So far will define in the briefest manner, U, T, Q, and activity.

Now let the F's vanish, so that no energy can be communicated to the system, whilst it can only leave it irreversibly, through Q. Then let p_1, p_2 be any two values of p satisfying (88) regarded as algebraic. Let Q_1, U_1, T_1 belong to the system p_1 existing alone; then, by (89) and (90),

$$0 = Q_1 + \dot{U}_1 + \dot{T}_1, \text{ or } 0 = Q_1 + 2p_1(U_1 + T_1);$$

$$0 = Q_2 + \dot{U}_2 + \dot{T}_2, \text{ ,, } 0 = Q_2 + 2p_2(U_2 + T_2).$$

But when existing simultaneously, so that

$$Q = Q_1 + Q_2 + Q_{12}, \quad U = U_1 + U_2 + U_{12}, \quad T = T_1 + T_2 + T_{12},$$

where U_{12}, T_{12}, Q_{12} depend upon products from both systems, thus :—

$$Q_{12} = 2\{B_{11}v_1v_1' + B_{22}v_2v_2' + B_{12}(v_1v_2' + v_2v_1') + \ldots\},$$

$$U_{12} = A_{11}x_1x_1' + A_{22}x_2x_2' + A_{12}(x_1x_2' + x_2x_1') + \ldots,$$

$$T_{12} = C_{11}v_1v_1' + C_{22}v_2v_2' + C_{12}(v_1v_1' + v_2v_1') + \ldots,$$

the accents distinguishing one system from the other, we shall find, by forming the equations of mutual activity $\Sigma Fv' = \ldots$, and $\Sigma F'v = \ldots$, that is, with the F's of one system, and the v's of the other, in turn,

$$\left.\begin{array}{l} 0 = \tfrac{1}{2}Q_{12} + p_2U_{12} + p_1T_{12}, \\ 0 = \tfrac{1}{2}Q_{12} + p_1U_{12} + p_2T_{12}; \end{array}\right\}$$

adding which, there results the equation of mutual activity,

$$0 = Q_{12} + (p_1 + p_2)(U_{12} + T_{12}), \text{ or } 0 = Q_{12} + \dot{U}_{12} + \dot{T}_{12};$$

and, on subtraction, there results

$$0 = (p_1 - p_2)(U_{12} - T_{12}), \quad \ldots \quad (91)$$

giving $U_{12} = T_{12}$, if the p's are unequal. But this property is true whether the p's be equal or not; that is, $U_{11} = T_{11}$ when p_1 is a repeated root. Various cases of the above are discussed in 'The Electrician,' November 27 and December 11, 1885, with special reference to the dynamical system expressed by Maxwell's electromagnetic equations.

The following applies to Maxwell's system, using the equations (4) to (10) of Part I. (Phil. Mag. August 1886). A comparison with the above is instructive. Let E_1, H_1 and E_2, H_2 be any two systems satisfying these equations, with no impressed forces, or $e = 0$, $h = 0$. Then the energy entering the unit volume per second by the action of the first system

on the second is

conv. $VE_1H_2/4\pi = (E_1 \text{ curl } H_2 - H_2 \text{ curl } E_1)/4\pi,$

$$= E_1\Gamma_2 + H_2G_1,$$

$$= E_1C_2 + E_1\dot{D}_2 + H_2\dot{B}_1/4\pi. \quad . \quad . \quad . \quad (92)$$

Similarly, by the action of the second system on the first,

conv. $VE_2H_1/4\pi = E_2C_1 + E_2\dot{D}_1 + H_1\dot{B}_2/4\pi. \quad . \quad . \quad (93)$

Addition gives the equation of mutual activity. And, subtracting (93) from (92), we find

conv. $(VE_1H_2 - VE_2H_1)/4\pi = (E_1\dot{D}_2 - E_2\dot{D}_1)$

$$- (H_1\dot{B}_2 - H_2\dot{B}_1)/4\pi ; \quad . \quad (94)$$

since $E_1C_2 = E_1kE_2 = E_2kE_1 = E_2C_1$, if there be no rotatory power, or C be a symmetrical linear function of E. Similarly for D and E, and B and H. Hence, if the systems are normal, making $d/dt = p_1$ in one, and p_2 in the other, (94) becomes

conv. $(VE_1H_2 - VE_2H_1)/4\pi = (p_2 - p_1)(E_1D_2 - H_1B_2/4\pi).$ (95)

Therefore, by the well-known theorem of Convergence, if we integrate through any region, and U_{12}, T_{12} be the mutual electric energy and the mutual magnetic energy of the two systems in that region, we obtain

$$U_{12} - T_{12} = \frac{\Sigma N(VE_2H_1 - VE_1H_2)/4\pi}{p_1 - p_2}, \quad . \quad . \quad (96)$$

where N is the unit normal drawn inward from the boundary of the region, over which the summation extends. And if the region include the whole space through which the systems extend, the right member will vanish, giving $U_{12} = T_{12}$, when these are complete.

From (96) we obtain, by differentiation, the value of twice the excess of the electric over the magnetic energy of a single normal system in any region ; thus

$$2(U - T) = \Sigma N \left(VE\frac{dH}{dp} - V\frac{dE}{dp} H \right)/4\pi. \quad . \quad (97)$$

This formula, or special representatives of the same, is very useful in saving labour in investigations relating to normal systems of subsidence.

The quantity that appears in the numerator in (96) is the excess of the energy entering the region through its boundary per second by the action of the second system on the first, over that similarly entering due to the action of the first on

the second system.   Bearing this in mind, we can easily form the corresponding formula in a less general case.   Suppose, for example, we have two fine wire terminals, a and b, that are joined through any electromagnetic and electrostatic combination which does not contain impressed forces, nor receive energy from without except by means of the current, say C, entering it at a and leaving it at b.   Let also V be the excess of the potential of a over that of b.   Then VC is the energy-current, or the amount of energy added per second to the combination through the terminal connections with, necessarily, some other combination.   (In the previous thick-letter vector investigation V was the symbol of vector product. There will, however, be no confusion with the following use of V, as in Part II., to express the line-integral of an electric force.   One of the awkward things about the notation in Prof. Tait's ' Quaternions ' is the employment of a number of most useful letters, as S, T, U, V, wanted for other purposes, as mere symbols of operations, putting another barrier in the way of practically combining vector methods with ordinary scalar methods, besides the perpetual negative sign before scalar products.)   The combination need not be of mere linear circuits, in which differences of current-density are insensible ; there may, for example, be induction of currents in a mass of metal not connected conductively with a and b, or the same mass may be in connection ; but in any case it is necessary that the arrangement should terminate in fine wires at a and b, in order that the two quantities V and C may suffice to specify, by their product, the energy-current at the terminals.   Even in this we completely ignore the dielectric currents and also the displacement, in the neighbourhood of the terminals, *i. e.* we assume $c=0$, to stop displacement. This is, of course, what is always done, unless specially allowed for.

Now supposing the structure of the combination to be given, we can always, by writing out the equations of its different parts, arrive at the characteristic equation connecting the terminal V and C.   For instance,

$$V = ZC, \quad \ldots \ldots \ldots \quad (98)$$

where Z is a function of d/dt.   In the simplest case Z is a mere resistance.   A common form of this equation is

$$f_0 V + f_1 \dot{V} + f_2 \ddot{V} + \ldots = g_0 C + g_1 \dot{C} + g_2 \ddot{C} + \ldots,$$

where the f's and g's are constants.   But there is no restriction to such simple forms.   All that is necessary is that the equa-

tion should be linear, so that Z may be a function of p. If, for example, $(dC/dt)^2$ occurred, we could not do it.

Now this combination must necessarily be joined on to another, however elementary, to make a complete system, unless V is to be zero always. The complete system, without impressed forces in it, has its proper normal modes of subsidence, corresponding to definite values of p. Consequently, by (96),

$$U_{12} - T_{12} = (V_2 C_1 - V_1 C_2) \div (p_1 - p_2), \quad . \quad . \quad (99)$$

if V_1, C_1 belong to p_1, and V_2, C_2 to p_2, whilst the left member refers to the combination given by $V = ZC$. Or

$$U_{12} - T_{12} = C_1 C_2 \left(\frac{V_1}{C_1} - \frac{V_2}{C_2} \right) \div (p_2 - p_1) = C_1 C_2 \frac{Z_1 - Z_2}{p_2 - p_1}, \quad (100)$$

and the value of $2(U - T)$ in a single normal system is

$$2(U - T) = V \frac{dC}{dp} - C \frac{dV}{dp} = -C^2 \frac{d}{dp} \frac{V}{C} = -C^2 \frac{dZ}{dp}. \quad . \quad (101)$$

In a similar manner we can write down the energy-differences for the complementary combination, whose equation is, say, $V = YC$; remembering that $-VC$ is the energy entering it per second, we get

$$C_1 C_2 \frac{Y_1 - Y_2}{p_1 - p_2} \text{ and } C^2 \frac{dY}{dp} \text{ respectively.}$$

By addition, the complete $U_{12} - T_{12}$ is

$$C_1 C_2 \frac{Y_1 - Y_2 - Z_1 + Z_2}{p_1 - p_2} = 0 = C_1 C_2 \frac{\phi_1 - \phi_2}{p_1 - p_2}; \quad . \quad (102)$$

and the complete $2(U - T)$ is

$$C^2 \frac{d}{dp} (Y - Z), \text{ or } C^2 \frac{d\phi}{dp}, \quad . \quad . \quad . \quad (103)$$

where $\phi = 0$, or $Y - Z = 0$, is the determinantal equation of the complete system (both combinations which join on at a and b, where V and C are reckoned), expressed in such a form that every term in ϕ is of the dimensions of a resistance.

If the complete system depends only upon a finite number of variables, it is clear that the number of independent normal systems is also finite, and there is no difficulty whatever in understanding how any possible initial state is decomposable into the finite number of normal states; nor is any proof needed that it is possible to do it. The constant A_1, fixing

the size of a particular normal system p_1, will be given by

$$A_1 = \frac{U_{01} - T_{01}}{U_{11} - T_{11}} = \frac{U_{01} - T_{01}}{2(U_1 - T_1)} = \frac{U_{01} - T_{01}}{C_1^2 \dfrac{d\phi}{dp_1}} \quad . \quad . \quad (104)$$

by the previous, if U_{01} be the mutual electric energy of the given initial state and the normal system, and T_{01} similarly the mutual magnetic energy.

And when we increase the number of variables infinitely, and pass to partial differential equations and continuously varying normal functions, it is, by continuity, equally clear that the decomposition of the initial state into the now infinite series of normal functions is not only possible, but necessary. Provided always that we have the whole series of normal functions at command. Therein lies the difficulty, when there is any.

In such a case as the system (71) of Part II., involving the partial differential equation

$$\frac{d^2 V}{dz^2} = RS \frac{dV}{dt} + LS \frac{d^2 V}{dt^2}, \quad . \quad . \quad . \quad (105)$$

wherein R, S, and L are constants, to hold good between the limits $z = 0$ and $z = l$, subject to

$$V = Z_0 C \text{ at } x = 0, \text{ and } V = Z_1 C \text{ at } x = l,$$

there is no possible missing of the true normal functions which arise by treating d/dt as a constant ; so that we can be sure of the possibility of the expansions. Thus, denoting $RSp + LSp^2$ by $-m^2$, we may take the normal V function as

$$u = \sin(mz + \theta), \quad . \quad . \quad . \quad . \quad (106)$$

and the corresponding normal C function as

$$w = +\frac{Sp}{m^2}\frac{du}{dz} = +\frac{Sp}{m}\cos(mz + \theta). \quad . \quad . \quad (107)$$

Here θ will be determined by the terminal conditions

$$\frac{u}{w} = Z_0 \text{ at } z = 0, \quad \frac{u}{w} = Z_1 \text{ at } z = l, \quad . \quad . \quad . \quad (108)$$

and the complete V and C solutions are

$$V = \Sigma A u \epsilon^{pt}, \quad C = \Sigma A w \epsilon^{pt} \quad . \quad . \quad . \quad . \quad (109)$$

at time t ; where any A is to be found from the initial state,

say V_0, C_0, functions of z, by

$$A = \frac{\int_0^l (SV_0 u - LC_0 w)dz}{\left[w^2 \frac{d}{dp} \left(\frac{u}{w} - Z \right) \right]_0^l}, \quad \cdot \quad \cdot \quad \cdot \quad (110)$$

provided there be no energy initially in the terminal arrangements. If there be, we must make corresponding additions to the numerator, without changing the denominator of A. The expression to be used for u/w is, by (106) and (107),

$$\frac{u}{w} = \frac{m}{Sp} \tan (mz + \theta), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (111)$$

remembering that m is a function of p. There are four components in the denominator of (110), as there are three electrical systems; viz. the terminal arrangements, which can only receive energy from the "line," and the line itself, which can receive or part with energy at both ends.

In a similar manner, if we make R, S, and L any single-valued functions of z, subject to the elementary relations of (71), Part II., or

$$-\frac{dV}{dz} = RC + L\dot{C}, \quad -\frac{dC}{dz} = S\dot{V}, \quad \cdot \quad \cdot \quad (112)$$

getting this characteristic equation of C,

$$\frac{d}{dz} \left(S^{-1} \frac{dC}{dz} \right) = \left(R + L \frac{d}{dt} \right) \frac{dC}{dt}, \quad \cdot \quad \cdot \quad \cdot \quad (113)$$

and, putting w for C and p for $\frac{d}{dt}$, this equation for the current function,

$$\frac{d}{dz} \left(S^{-1} \frac{dw}{dz} \right) = (R + Lp)pw, \quad \cdot \quad \cdot \quad \cdot \quad (114)$$

and finding the u functions by the second of (112), giving

$$-Spu = \frac{dw}{dz}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (115)$$

we see that the expansions of the initial states V_0 and C_0 can be effected, subject to the terminal conditions (108). For the normal potential and current functions will be perfectly definite (singularities, of course, to receive special attention), given by (113) and (114), as each the sum of two independent functions, and the terminal conditions will settle in what ratio they must be taken. (109) and (110) will constitute

the solution, except as regards the initial energy beyond the terminals.

It is, however, remarkable, that we can often, perhaps universally, find the expression for the part of the numerator of (110) to be added for the terminal arrangements, except as regards arbitrary multipliers, from the mere form of the Z functions, without knowing in detail what electrical combinations they represent. This is to be done by first decomposing the expression for $C^2(dZ/dp)$ into the sum of squares, for instance,

$$C^2 \frac{dZ}{dp} = r_1\{f_1(p)\}^2 + r_2\{f_2(p)\}^2 + \dots, \quad . \quad . \quad (116)$$

where $r_1, r_2, \dots$ are constants. The terminal arbitraries are then $\Sigma A f_1(p)$, $\Sigma A f_2(p)$, &c. : calling these $E_1, E_2, \dots$, the additions to the numerator of (110) are

$$- \{E_1 r_1 f_1(p) + E_2 r_2 f_2(p) + \dots\}, \quad . \quad . \quad (117)$$

wherein the E's may have any values. This must be done separately for each terminal arrangement. The matter is best studied in the concrete application, which I may consider under a separate heading.

It is also remarkable that, as regards the obtaining of correct expansions of functions, there is no occasion to impose upon R, S, and L the physical necessity of being positive quantities, or real. This will be understandable by going back to a finite number of variables, and then passing to continuous functions.

Let us now proceed to the far more difficult problems connected with propagation along a dielectric tube bounded by concentric conducting tubes, and examine how the preceding results apply, and in what cases we can be sure of getting correct solutions. Start with the general system, equations (11) to (14), Part I., with the extension mentioned at the commencement of Part II. from a solid to a tubular inner conductor. Suppose that the initial state is of purely longitudinal electric force, independent of z, so that the longitudinal E and circular H are functions of r only. How can we secure that they shall, in subsiding, remain functions of r only, so that any short length is representative of the whole? Since E is to be longitudinal, there must be no longitudinal energy-current, or it must be entirely radial. Therefore no energy must be communicated to the system at $z=0$ or $z=l$, or leave it at those places. This seems to be securable in only five cases. Put infinitely conducting plates across the section at either or both ends of the line. This will make $V=0$ there,

if V is the line-integral of the radial electric force across the dielectric. Or put non-conducting and non-dielectric plates there similarly. This will make $C = 0$. Or, which is the fifth case, let the inner and the outer conductors be closed upon themselves. In any of these cases, the electric force will remain longitudinal during the subsidence, which will take place similarly all along the line. By (14), the equation of H will be

$$\frac{d}{dr}\frac{1}{r}\frac{d}{dr}\,rH = 4\pi k\mu\dot{H} + \mu c\dot{H} \; ;$$

and it is clear that the normal functions are quite definite, so that the expansion of the initial state of E and H can be truly effected. In the already given normal functions take $m = 0$.

But if we were to join the conductors at one end of the line through a resistance, we should, to some extent, upset this regular subsidence everywhere alike. For energy would leave the line; this would cause radial displacement, first at the end where the resistance was attached, and later all along the line. (By "the line" is meant, for brevity, the system of tubes extending from $z = 0$ to $z = l$.)

Now in short-wire problems the electric energy is of insignificant importance, as compared with the magnetic. It is usual to ignore it altogether. This we can do by assuming $c = 0$. This necessitates equality of wire and return current, for one thing; but, more importantly, it prevents current leaving the conductors, so that C and H and Γ the current-density, are independent of z. There will be no radial electric force in the conductors, in which therefore the energy-current will be radial. But there will be radial force in the dielectric, and therefore longitudinal energy-current. Since the radial electric force and also the magnetic force in the dielectric vary inversely as the distance from the axis, the longitudinal energy-current density will vary inversely as the square of the distance. But, on account of symmetry, we are only concerned with its total amount over the complete section of the dielectric. This is

$$\frac{1}{4\pi}\int_{a_1}^{a_2}\frac{2C}{r}\,.\,E_r\,.\,2\pi r\,dr = VC, \quad . \quad . \quad . \quad (118)$$

if V is the line-integral of E_r the radial force, and C the wire-current. It is clear, then, that we can now allow terminal connections of the form $V/C = Z$ before used, and still have correct expansions of the initial magnetic field, giving correct subsidence solutions.

But it is simpler to ignore V altogether. For the equation

of E.M.F. will be

$$e_0 = (Z_0 + Z_1 + lL_0p + lR_1'' + lR_2'')C, \quad . \quad . \quad (119)$$

if e_0 is the total impressed force in the circuit, R_1'' and R_2'' the wire and sheath functions of equations (55) and (56), Part II., on the assumption $m = 0$, and Z_0, Z_1 the terminal functions, such that $V/C = Z_1$ at $z = l$, and $= -Z_0$ at $z = 0$. It does not matter how e_0 is distributed so far as the magnetic field and the current is concerned. Let it then be distributed in such a way as to do away with the radial electric field, for simplicity of reasoning. The simple-harmonic solution of (119) is obviously to be got by expanding Z_0 and Z_1 in the form $R + Lp$, where R and L are functions of p^2, and adding them on to the $l(R' + L'p)$ equivalent of $l(L_0p + R_1'' + R_2'')$, as in equation (66), Part II.

Regarding the free subsidence, putting $e_0 = 0$ in (119) gives us the determinantal equation of the p's; and as the normal H functions are definitely known, the expansion of the magnetic field can be effected. The influence of the terminal arrangements must not be forgotten in reckoning A.

In coming, next, to the more general case of equation (56), but without restriction to exactly longitudinal current in the conductors, it is necessary to consider the transfer of energy more fully. In the dielectric the longitudinal energy-current is still VC. The rate of decrease of this quantity with z is to be accounted for by increase of electric and magnetic energy in the dielectric, and by the transfer of energy into the conductors which bound it. Thus,

$$-\frac{d}{dz}VC = -\frac{dV}{dz}C - \frac{dC}{dz}V.$$

But here,

$$-\frac{dC}{dz} = S\dot{V}, \text{ and } -\frac{dV}{dz} = L_0\dot{C} + E - F, \quad . \quad (120)$$

by (59) and (56), Part II., E and F being the longitudinal electric forces at the inner and outer boundaries of the dielectric (when there is no impressed force). So

$$-\frac{d}{dz}VC = SV\dot{V} + L_0C\dot{C} + EC - FC. \quad . \quad . \quad (121)$$

The first term on the right side is the rate of increase of the electric energy, the second term the rate of increase of the magnetic energy in the dielectric, the third is the energy entering the inner conductor per second, the fourth that entering the outer conductor; all per unit length.

If the electric current in the conductors were exactly lon-

gitudinal, the energy-transfer in them would be exactly radial, and EC and −FC would be precisely equal to the Joule heat per second *plus* the rate of increase of the magnetic energy, in the inner and the outer conductor, respectively. But as there is a small radial current, there is also a small longitudinal transfer of energy in the conductors. Thus, E_r and E_z being the radial and longitudinal components of the electric force, in the inner conductor, for example, the longitudinal and the radial components of the energy-current per unit area are

$$E_r H/4\pi \text{ and } E_z H/4\pi,$$

the latter being inward. Their convergences are

$$-\frac{d}{dx}\frac{E_r H}{4\pi}, \text{ and } \frac{1}{r}\frac{d}{dr} r \frac{E_z H}{4\pi},$$

or

$$\frac{E_r}{4\pi}\left(-\frac{dH}{dz}\right)-\frac{H}{4\pi}\frac{dE_r}{dz}, \text{ and } \frac{E_z H}{4\pi}+\frac{E_z}{4\pi}\frac{dH}{dr}+\frac{H}{4\pi}\frac{dE_z}{dr},$$

or

$$E_r \Gamma_r -\frac{H}{4\pi}\frac{dE_r}{dz}, \text{ and } E_z \Gamma_z +\frac{H}{4\pi}\frac{dE_z}{dr},$$

if Γ_r and Γ_z are the components of the electric current-density. The sum of the first terms is clearly the dissipativity per unit volume; and that of the second terms is, by equation (13), Part I., $H\mu\dot{H}/4\pi$, the rate of increase of the magnetic energy.

The longitudinal transfer of energy in either conductor per unit area is also expressed by $-(4\pi k)^{-1}H(dH/dz)$; or, by $-(4\pi k\mu)^{-1}(dT_1/dz)$ across the complete section, if T_1 temporarily denote the magnetic energy in the conductor per unit length.

Now let E_1, F_1, C_1, V_1, and E_2, F_2, C_2, V_2 refer to two distinct normal systems. Then, if we could neglect the longitudinal transfer in the conductors, we should have

$$U_{12}-T_{12}=\frac{d}{dz}(V_1 C_2 - V_2 C_1)\div(p_1-p_2), \quad . \quad . \quad (122)$$

the left side referring to unit length of line; and, in the whole line,

$$U_{12}-T_{12}=[V_1 C_2 - V_2 C_1]_0^l \div(p_1-p_2). \quad . \quad . \quad (123)$$

Similarly, for a single normal system,

$$2(U-T)=\frac{d}{dz}C^2\frac{d}{dp}\frac{V}{C}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (124)$$

per unit length ; and, in the whole line

$$2(\mathrm{U}-\mathrm{T}) = \left[\mathrm{C}^2\frac{d}{dp}\frac{\mathrm{V}}{\mathrm{C}}\right]_0^l. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (125)$$

We have to see how far these are affected by the longitudinal transfer.   We have

$$-\frac{d}{dz}\mathrm{V}_1\mathrm{C}_2 = \mathrm{SV}_1\dot{\mathrm{V}}_2 + \mathrm{L}_0\mathrm{C}_2\dot{\mathrm{C}}_1 + (\mathrm{E}_1-\mathrm{F}_1)\mathrm{C}_2,$$

$$-\frac{d}{dz}\mathrm{V}_2\mathrm{C}_1 = \mathrm{SV}_2\dot{\mathrm{V}}_1 + \mathrm{L}_0\mathrm{C}_1\dot{\mathrm{C}}_2 + (\mathrm{E}_2-\mathrm{F}_2)\mathrm{C}_1 ;$$

therefore, if the systems are normal,

$$\frac{d}{dz}(\mathrm{V}_1\mathrm{C}_2 - \mathrm{V}_2\mathrm{C}_1) = (p_1-p_2)(\mathrm{SV}_1\mathrm{V}_2 - \mathrm{L}_0\mathrm{C}_1\mathrm{C}_2)$$
$$- (\mathrm{E}_1-\mathrm{F}_1)\mathrm{C}_2 + (\mathrm{E}_2-\mathrm{F}_2)\mathrm{C}_1.$$

It will be found that we cannot make the parts depending upon E and F exactly represent the $\mathrm{U}_{12}-\mathrm{T}_{12}$ in the conductors except when m^2 is the same in both systems p_1 and p_2. In that case, the parts $(\mathrm{E}_r)_1(\mathrm{H})_2$ and $(\mathrm{E}_r)_2(\mathrm{H})_1$ of the longitudinal transfer of energy in the conductors, depending upon the mutual action of the two systems, are equal ; $(\mathrm{E}_r)_1$ and $(\mathrm{E}_r)_2$ being proportional to $\sin mz$, and H_1 and H_2 proportional to $\cos mz$.   So, in case p_1 and p_2 are values of p belonging to the same m^2, the influence of the longitudinal energy-transfer in the conductors goes out from (122) and (123), which are therefore true in spite of it.   Similarly, provided the m's can be settled independently of the p's, equations (124) and (125) are true.

Now the normal V and C functions, say u and w, as before, may be taken to be

$$u = \frac{m}{\mathrm{S}p}\left\{\tfrac{1}{2}a_1\mathrm{J}_1(s_1a_1) - \tfrac{1}{2}a_1(\mathrm{J}_1/\mathrm{K}_1)(s_1a_0)\mathrm{K}_1(s_1a_1)\mathrm{K}_1(s_1a_1)\right\}\sin(mz+\theta) \left.\right\}$$

$$w = \quad\left\{\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad.\quad\right\}\cos(mz+\theta) \left.\right\}$$

so that $\mathrm{V}=\mathrm{A}u\epsilon^{pt}$, $\mathrm{C}=\mathrm{A}w\epsilon^{pt}$; and

$$\frac{\mathrm{V}}{\mathrm{C}} = \frac{u}{w} = \frac{m}{\mathrm{S}p}\tan(mz+\theta); \quad \cdot \quad \cdot \quad \cdot \quad (127)$$

and the complete equations for the determination of m, θ, and p are

$$\left.\begin{array}{c}\dfrac{m}{\mathrm{S}p}\tan\theta = \mathrm{Z}_0, \quad \dfrac{m}{\mathrm{S}p}\tan(ml+\theta) = \mathrm{Z}_1, \\[2mm] 0 = \dfrac{m^2}{\mathrm{S}p} + \mathrm{R}'_m + \mathrm{L}'_m p ; \end{array}\right\} \quad \cdot \quad (128)$$

the first two of these being the terminal conditions, and $R'_m + L'_m p$ being merely a convenient way of writing the real complex expressions; (equation (68), with $e_m = 0$). It is clear that the only cases in which the m's become clear of the p's are the before-mentioned five cases, equivalent to Z_0 and Z_1 being zero or infinite, and the line closed upon itself, which is a sort of combination of both. Considering only the four, they are summed up in this, $VC = 0$ at the terminals, or the line cut off from receiving or losing energy at the ends. We have then the series of m's, $0, \pi/l, 2\pi/l, \&c.$; or $\frac{1}{2}\pi/l, \frac{3}{2}\pi/l, \frac{5}{2}\pi/l$, &c.; and every m^2 has its own infinite series of p's through the third equation (128). These, though very special, are certainly important cases, as well as being the most simple. We can definitely effect the expansions of the initial states in the normal functions, and obtain the complete solutions in every particular.

Although rather laborious, it is well to verify the above results by direct integration of the proper expressions for the electric and magnetic energies of normal systems throughout the whole line. Thus, let

$$\frac{d}{dr}\frac{1}{r}\frac{d}{dr}rH_1 + s_1^2 H_1 = 0, \text{ where } -s_1^2 = 4\pi\mu_1 k_1 p_1 + m_1^2,$$

$$\frac{d}{dr}\frac{1}{r}\frac{d}{dr}rH_2 + s_2^2 H_2 = 0, \text{ where } -s_2^2 = 4\pi\mu_1 k_1 p_2 + m_2^2,$$

in the inner conductor. We shall find

$$(s_1^2 - s_2^2)\int_{a_0}^{a_1} H_1 H_2 r\, dr = 8\pi(C_1\Gamma_2 - C_2\Gamma_1),$$

as $H_1 = 0 = H_2$ at $r = a_0$; Γ_1 and Γ_2 being the longitudinal current-densities at $r = a_1$. Similarly for the outer conductor,

$$(s_1'^2 - s_2'^2)\int_{a_2}^{a_3} H'_1 H'_2 r\, dr = -8\pi(C_1\Gamma'_2 - C_2\Gamma'_1)$$

if C_1, C_2 still be the currents in the inner conductor; the accents merely meaning changes produced by the altered μ and k in the outer conductor. $H_1' = 0 = H_2'$ at $r = a_3$ in this case. Then, thirdly, for the intermediate space,

$$\int_{a_1}^{a_2} H_1'' H_2'' r\, dr = C_1 C_2 \times 4 \log \frac{a_2}{a_1}.$$

Therefore the total mutual magnetic energy of the two distri-

butions per unit length is

$$\frac{\mu_1}{4\pi}\int_{a_0}^{a_1} H_1 H_2 \cdot 2\pi r dr + \frac{\mu_2}{4\pi}\int_{a_1}^{a_2} H_1'' H_2'' \cdot 2\pi r dr$$

$$+ \frac{\mu_3}{4\pi}\int_{a_2}^{a_3} H_1' H_2' \cdot 2\pi r dr,$$

which, by using the above expressions, becomes, provided $m_1^2 = m_2^2$,

$$L_0 C_1 C_2 - \frac{C_1(E_2 - F_2)}{p_1 - p_2} + \frac{C_2(E_1 - F_1)}{p_1 - p_2}, \quad . \quad . \quad (126a)$$

E and F being Γ/k or the longitudinal electric forces at $r = a_1$ or $r = a_2$. But

$$E - F = R'' C,$$

where $R'' =$ the $R_1'' + R_2''$ of equation (56), Part II.; and

$$0 = \frac{m^2}{Sp} + L_0 p + R'' = \frac{m^2}{Sp} + R' + L'p,$$

so (126) becomes

$$\left(C_1 \frac{dV_2}{dz} - C_2 \frac{dV_1}{dz}\right) \div (p_1 - p_2), \quad \text{or} \quad \frac{m^2 C_1 C_2}{Sp_1 p_2}. \quad . \quad (127a)$$

The mutual electric energy is obviously $SV_1 V_2$ per unit length. By summation with respect to z from 0 to l, subject to $VC = 0$ at both ends, we verify that the total mutual magnetic energy equals the total mutual electric energy. The value of $2T$ in a single normal system is, by $(126a)$, and the next equation,

$$L_0 C^2 + C^2 \frac{dR''}{dp} = C^2 \frac{d}{dp}(R' + L'p) \quad . \quad . \quad (128a)$$

per unit length; and that of $2U$ is SV^2. Hence, per unit length,

$$2(U - T) = SV^2 - C^2 \frac{d}{dp}(R' + L'p). \quad . \quad (129)$$

In this use $V = u$ and $C = w$, equations (126), and we shall obtain, for the complete energy-difference in the whole line,

$$-\left\{\frac{a_1}{2}J_1(s_1 a_1) - \dots\right\}^2 \frac{l}{2}\frac{d}{dp}\left(\frac{m^2}{Sp} + R' + L'p\right) = M \text{ say,} \quad (130)$$

which is the expanded form of

$$\left[w\frac{du}{dp} - u\frac{dw}{dp}\right]_0^l \quad \text{or} \quad \left[w^2\frac{d}{dp}\left(\frac{u}{w} - Z\right)\right]_0^l,$$

as may be verified by performing the differentiations, using the expression for u/w in (127), remembering that m^2 in it is

a function of p; or more explicitly, put $\sqrt{-\mathrm{S}p(\mathrm{R}'+\mathrm{L}'p)}$ for m, and then differentiate to p.

Given, then, the initial state to be $\mathrm{V}=\mathrm{V}_0$, a function of z, and $\mathrm{H}=\mathrm{H}_{01}$ in the inner conductor, H_{02} in the dielectric, and H_{03} in the outer conductor, functions of r and z, and that this system is left without impressed force, subject to $\mathrm{VC}=0$ at both ends, the state at time t later will be given by

$$\mathrm{V}=\Sigma\mathrm{A}u\epsilon^{pt}, \quad \mathrm{C}=\Sigma\mathrm{A}w\epsilon^{pt};$$

the summations to include every p, with similar expressions for H, Γ, γ, &c., the magnetic force and two components of current, by substituting for u or w the proper corresponding normal functions; the coefficient A being given by the fraction whose denominator is the expression M in (130), and whose numerator is the excess of the mutual electric energy of the initial and the normal system over their mutual magnetic energy, expressed by

$$\frac{m}{p}\,\mathrm{C}'\int_0^l\mathrm{V}_0\sin{(mz+\theta)}dz$$
$$-\int_0^l\cos{(mz+\theta)}\,dz\left\{\int_{a_0}^{a_1}\mu_1\mathrm{H}_{01}\mathrm{C}_1'dr+\int_{a_1}^{a_2}\mu_2\mathrm{H}_{02}\mathrm{C}'dr\right.$$
$$\left.+\int_{a_2}^{a_3}\mu_3\mathrm{H}_{03}\mathrm{C}_3'dr\right\}, \quad . \quad (131)$$

where $\quad \mathrm{C}'=\dfrac{a_1}{2}\left\{\mathrm{J}_1(s_1a_1)-(\mathrm{J}_1/\mathrm{K}_1)(s_1a_0)\mathrm{K}_1(s_1a_1)\right\};$

and C_1' is the same with r put for a_1, and C_3' is the same with r put for a_1, a_3 for a_0, and s_3 for s_1. It should not be forgotten that in the case $m=0$, the denominator (130) requires to be doubled, $\frac{1}{2}l$ becoming l. Also that R'', or $\mathrm{R}'+\mathrm{L}'p$, contains m^2, and must not be the $m=0$ expressions for the same.

To check, take the initial state to be $e_0(1-z/l)$, with no magnetic force, and that $\mathrm{V}=0$ at both ends. We find immediately, by (130) and (131), that at time t,

$$\mathrm{V}=\frac{2e_0}{l}\,\Sigma\,\frac{1}{m}\sin mz\,\Sigma\,\frac{(m^2/\mathrm{S}p^2)\epsilon^{pt}}{-\dfrac{d}{dp}\left(\dfrac{m^2}{\mathrm{S}p}+\mathrm{R}'+\mathrm{L}'p\right)}, \quad . \quad (132)$$

where the m's are to be π/l, $2\pi/l$, $3\pi/l$, &c.; the first summation being with respect to m, and the second for the p's of a particular m.

But, initially,

$$\mathrm{V}=e_0\left(1-\frac{z}{l}\right)=\frac{2e_0}{l}\,\Sigma\,\frac{1}{m}\sin mz.$$

Therefore we must have

$$1 = \Sigma \frac{m^2/\mathbb{S}p^2}{-\dfrac{d}{dp}\left(\dfrac{m^2}{\mathbb{S}p} + \mathrm{R}' + \mathrm{L}'p\right)}.$$

Simplified, it makes this theorem

$$-\frac{1}{\phi(0)} = \Sigma\left(p\,\frac{d\phi}{dp}\right)^{-1},$$

if the p's are the roots of $\phi(p)=0$. This is correct.

To determine the effect of longitudinal impressed force, keeping to the case of uniform intensity over the cross section of either conductor. Let a steady impressed force of integral amount e_0 be introduced in the line at distance z_1; it may be partly in one and partly in the other conductor, as in Part II. By elementary methods, we can find the steady state of V, C it will set up. If, then, we remove e_0, we can, by the preceding, find the transient state that will result. Let V_0 be the steady state of V set up, and V_1 what it becomes at time t after removal of e_0; then $\mathrm{V}_0 - \mathrm{V}_1$ represents the state at time t after e_0 is put on. So, if $\Sigma \mathrm{A}u$ represent the V set up by the unit impressed force at z_1,

$$\mathrm{V} = \mathrm{V}_0 - e_0 \Sigma \mathrm{A}u\epsilon^{pt}$$

will give the distribution of V at time t after e_0 is put on, being zero when $t=0$, and V_0 when $t=\infty$. No zero value of p is admissible here.

From this we deduce that the effect of e_0 lasting from $t=t_1$ to $t=t_1 + dt_1$ at the later time t is

$$-\Sigma \mathrm{A}up e_0 dt_1 \epsilon^{p(t-t_1)};$$

therefore, by time integration, the effect due to an impressed force e_0 at one spot, variable with the time, starting at time t_0 is

$$\mathrm{V} = -\Sigma \mathrm{A}up\epsilon^{pt}\int_{t_0}^{t} e_0 \epsilon^{-pt_1}dt_1,$$

in which e_0 is a function of t_1.

By integrating along the line, we find the effect of a continuously distributed impressed force, e per unit length, to be

$$\mathrm{V} = -\Sigma up\epsilon^{pt}\int_{0}^{l}\int_{t_0}^{t} \mathrm{A}e\epsilon^{-pt_1}dz_1 dt_1, \quad . \quad . \quad (133)$$

wherein e is a function of both z_1 and t_1, and starts at time t_0; whilst A is a function of z_1, the position of the elementary impressed force $\dot{e}dz_1$.

To find A as a function of z_1, we might, since ΣAu is the V set up by unit e at z_1, expand this state by the former process of integration. But the following method, though unnecessary for the present purpose, has the advantage of being applicable to cases in which VC is not zero at the terminals, but $V = ZC$ instead. It is clear that the integration process, including the energy in the terminal apparatus, would be very lengthy, and would require a detailed knowledge of the terminal combinations. This is avoided by replacing the impressed force at z_1 by a charged condenser; when, clearly, the integration is confined to one spot. Let S_1 be the capacity and V_0 the difference of potential of a condenser inserted at z_1. If we increase S_1 infinitely it becomes mathematically equivalent to an impressed force V_0, without the condenser.

Suppose $\Sigma Aw'\epsilon^{pt}$ is the current at z at time t after the introduction of the condenser, of finite capacity; then, since $-S_1\dot{V}$ is the current leaving the condenser, or the current at z_1, we have

$$-S_1\dot{V} = \Sigma Aw_1'\epsilon^{pt},$$

w_1' being the value of w' at z_1. The expansion of V_0 is therefore

$$V_0 = -\Sigma Aw_1'/S_1 p,$$

initially; and the mutual potential energy of the initial charge of the condenser and of the normal u' corresponding to w' must be

$$S_1V_0(-w_1'/S_1p) = -V_0w_1'/p.$$

But since there is, initially, electric energy only at z_1, and magnetic energy nowhere at all, the only term in the numerator of A will be that due to the condenser, or this $-V_0w_1'/p$; hence

$$A = -V_0w_1/pM,$$

where M is the $2(U-T)$ of the complete normal system, as modified by the presence of the condenser, is the value of A in $V = \Sigma Au'\epsilon^{pt}$, making

$$V = -V_0\Sigma(w_1'/pM)u'\epsilon^{pt},$$

expressing the effect at time t after the introduction of the condenser, and due to its initial charge.

So far S_1 has been finite, and consequently u', w', M, and p depend on its capacity as well as on the line and terminal conditions. But on infinitely increasing its capacity, u' and w' become u and w, the same as if the condenser were non-existent. Therefore

$$V = -\Sigma V_0(w_1/pM)u\epsilon^{pt} \quad . \quad . \quad . \quad (134)$$

expresses the effect due to the steady impressed force V_0 at z_1 at time t after it was started. This will have a term corresponding to a zero p (due to the infinite increase of S_1 in the previous problem), expressing the final state. Hence, leaving out this term, the summation (134), with sign changed, and $t = 0$, expresses the final state itself. Thus, taking $V_0 = 1$,

$$\Sigma \mathrm{A} u = \Sigma w_1 u / p \mathrm{M}$$

is the expansion required to be applied to (133). Put $\mathrm{A} = w_1/p\mathrm{M}$ in it, and it becomes

$$V = \Sigma (u/\mathrm{M}) \epsilon^{pt} \int_0^l \int_{t_0}^t w_1 e \epsilon^{-pt_1} dz_1 dt_1, \quad . \quad . \quad . \quad (135)$$

fully expressing the effect at z, t, due to the impressed force e, a function of z_1 and t_1, starting at time t_0. To obtain the current, change u to w outside the double integral. The M, when the condition $VC = 0$ at the ends is imposed, is that of (130) ; the u and w expressions those of (126). But if we regard S, R′, and L′ as constants (or functions of z), then (135) holds good when terminal conditions $V = ZC$ are imposed, provided the impressed force be in the line only, as supposed in (135).

When the impressed force is steady, and is confined to the place $z = 0$, and is of integral amount e_0, (135) gives

$$V = e_0 \Sigma u w_0 / p \mathrm{M} - e_0 \Sigma u w_0 \epsilon^{pt} / p \mathrm{M}, \quad . \quad . \quad . \quad (136)$$

w_0 being the value of w at $z = 0$, as the effect at time t after starting e_0. The first summation expresses the state finally arrived at.

Again, in (135) let the impressed force be a simple harmonic function of the time. I have already given the solution in this case, so far as the formula for C is concerned, in the case $V = 0$ at both ends, in equation (76), Part II., which may be derived from (135), by using in it w instead of u at its commencement, putting $e = e_0 \sin nt$, and effecting some reductions. The V formula may be got in a similar manner to that used in getting (76), but it is instructive to derive it from (135), as showing the inner meaning of that formula. Let in it $e = e_0 \sin (nt + \alpha)$, where e_0 is a function of z. Effect the t_1 integration, with $t_0 = 0$ for simplicity. The result is

$$V = -\Sigma \frac{u \epsilon^{pt}}{\mathrm{E}} \left(\frac{p \sin \alpha + n \cos \alpha}{p^2 + n^2} \right) \int_0^l w_1 e_0 dz_1$$

$$\Sigma + \frac{u}{\mathrm{M}} \left(\frac{p \sin (nt + \alpha) + n \cos (nt + \alpha)}{p^2 + n^2} \right) \int_0^l w_1 e_0 dz_1. \quad (137)$$

The first summation cancels the second at the first moment, and ultimately vanishes, leaving the second part to represent the final periodic solution. Take $\alpha = 0$; and use the u, w, M expressions of (126) and (130), and let ϕ_m stand for $m^2 + Sp(R'_m + L'_m p)$, so that $\phi_m = 0$ gives the p's for a particular m^2. Then we obtain, (with $V = 0$ at both ends),

$$V = \frac{d}{dz} \Sigma \frac{\cos mz \int_0^l \cos mz_1 \cdot e_0 dz_1 \cdot (p \sin nt + n \cos nt)}{\frac{1}{2} l \frac{d\phi_m}{dp} (p^2 + n^2)}$$

$$= \frac{d}{dz} \frac{2}{l} \Sigma \frac{\cos mz \int_0^l \cos mz_1 \cdot e_0 \sin nt \cdot dz_1}{\left(\frac{d}{dt} - p\right) \frac{d\phi_m}{dp}}, \quad . \quad . \quad . \quad (138)$$

because $d^2/dt^2 = -n^2$. But, if $e_0 = \Sigma e_m$, the equation of V_m is

$$-\phi_m V_m = \frac{de_m}{dz} \sin nt,$$

(by (60) and (63), Part II.), so that

$$V_m = -\phi_m^{-1} \frac{de_m}{dz} = -\frac{d}{dz} \Sigma \frac{e_m \sin nt}{\left(\frac{d}{dt} - p\right) \frac{d\phi}{dp}}, \quad . \quad (139)$$

by a well-known algebraical theorem, the summation being with respect to the p's, which are the roots of $\phi_m = 0$, considered as algebraic. We have also

$$e_0 = \frac{2}{l} \Sigma \cos mz \int_0^l \cos mz_1 e_0 dz_1, \quad . \quad . \quad . \quad (140)$$

the summation being with respect to m.

Uniting (139) and (140), there results the previous equation (138), in which the summation is with respect to all the p's belonging to all the m's. In the case $m = 0$, the $2/l$ must be halved. In the form of a summation with respect to m, similar to (77) for C, the corresponding V solution is

$$V = -\frac{2V_0}{Snl} \Sigma \frac{m \sin mz \{(L'_m - m^2/Sn^2)n \sin nt + R'_m \cos nt\}}{R_m^2 + (L_m - m^2/Sn^2)^2 n^2},$$

the impressed force being $V_0 \sin nt$, at $z = 0$. This, on the assumption $R'_m = R'$, $L'_m = L'$, will be found to be the expansion of the form (80), Part II.

Now to make some remarks on the impossibility of joining on terminal apparatus without altering the normal functions, the terminal arrangements being made to impose conditions of the form $V = ZC$. It is clear, in the first place, that if the quantity VC at $z = 0$ and $z = l$ really represents the energy-transfer in or out of the line at those places, then the equation

$$A_1 = \frac{U_{01} - T_{01}}{\left[w^2 \dfrac{d}{dp} \left(\dfrac{u}{w} - Z \right) \right]_0^l}$$

will be valid, provided u and w be the correct normal functions. But to make VC be the energy-transfer at the ends requires us to stop the longitudinal transfer in the conductors there, or make the current in the conductors longitudinal. This condition is violated when the current function w is proportional to $\cos(mz + \theta)$, as in the previous, except in the special cases, because the radial current γ in the conductors is proportional to $\sin(mz + \theta)$, and γ has to vanish. Not in the dielectric, but merely in the conductors.

We can ensure that VC is the energy-transfer at the ends by coating the conductors over their exposed sections with infinitely conducting material and joining the terminal apparatus on to the latter. The current in the conductors will be made strictly longitudinal, close up to the infinitely conducting material, and γ will vanish in the conductors. But γ in the dielectric at the same place will be continuous with the radial surface-current on the infinitely conducting ends, due to the sudden discontinuity in the magnetic force. Thus the energy-transfer, at the ends, is confined to the dielectric.

It is clear, however, that the normal current-functions in the two conductors must be such as to have no radial components at the terminals, so that they cannot be what have been used, such that $d^2/dz^2 = $ constant. They require alteration, of sensible amount may be, only near the terminals, but theoretically, all along the line. It would therefore appear that only the five cases of $V = 0$ at either or both ends, or $C = 0$ ditto, or the line closed upon itself, admit of full solution in the above manner. The only practical way out of the difficulty is to abolish the radial electric current in the conductors, making (66) the equation of V, and VC the longitudinal energy-transfer, with full applicability of the $V = ZC$ terminal conditions. With a further consideration of this system, and some solutions relating to it, I propose to conclude this paper.

XLII. *On Stationary Waves in Flowing Water.*—Part I.
By Sir WILLIAM THOMSON [*].

THIS subject includes the beautiful wave-group produced by a ship propelled uniformly through previously still water, but the present communication[†] is limited to two-dimensional motion.

Imagine frictionless water flowing in uniform regime through an infinitely long canal with vertical sides ; and bottom horizontal except where modified by transverse ridges or hollows, or slopes between portions of horizontal bottom at different levels. Included among such inequalities we may suppose bars above the bottom, fixed perpendicularly between the sides. Let these inequalities be all within a finite portion, AB, of the length, and let f denote the difference of levels of the bottom on the two sides of this position, positive if the bottom beyond A is higher than the bottom beyond B.

Now, let the water be given at an infinite, or very great, distance beyond A, perpetually flowing towards A with any prescribed constant velocity u, and filling up the canal to a prescribed constant depth a. It is required to find the motion of the water towards A, through AB, and beyond B as disturbed by the inequalities between A and B. This problem is essentially determinate; and it has only one solution if we confine it to cases in which the vertical component of the water's velocity is everywhere small in comparison with the velocity acquired by a falling body falling from a height equal to half the depth. Let b be the mean depth, and v the mean horizontal velocity at very great distances beyond B; and (to have w to denote wave-energy) let w be such that

$$(\tfrac{1}{2}v^2 + \tfrac{1}{2}gb)b + w \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is the whole energy, kinetic and potential, per unit of the canal's breadth and per unit of its length. In cases in which the water flows away unruffled at great distances from B, w is zero. But, in general, the surface is ruffled, and the water flows " *steadily* " between the plane bottom and a corrugated free surface, as in the well-known appearance of water flowing in a mill-lead, or Highland burn, or in the clear rivulet

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on the east side of Trumpington Street, Cambridge, or in the race of Portland or Islay overfalls. The train of diminishing waves which we see in the wake of each little irregularity of the bottom would, of course, extend to infinity if the stream were infinitely long, and the water absolutely inviscid (frictionless); and a single inequality, or group of inequalities, in any part AB of the stream would give rise to corrugation in the whole of the flow after passing the inequalities, more and more nearly uniform, and with ridges and hollows more and more nearly perpendicular to the sides of the canal, the farther we are from the last of the inequalities. Observation, with a little common sense of the mathematical kind, shows that at a distance of two or three wave-lengths from the last of the irregularities if the breadth of the canal is small in comparison with the wave-length, or at a distance of nine or ten breadths of the canal if the breadth is large in comparison with the wave-length, the condition of uniform corrugations with straight ridges perpendicular to the sides of the canal, would be fairly well approximated to; even though the irregularity were a single projection or hollow in the middle of the stream. But the subject of the present communication is simpler, as it is limited to two-dimensional motion; and our inequalities are bars, or ridges, or hollows, perpendicular to the sides of the canal. Thus, in our present case, we see that the condition of ultimate uniformity of the standing waves in the wake of the irregularities is closely approximated to at a distance of two or three wave-lengths from the last of the inequalities.

Let SA, SB denote two fixed vertical sections of the canal at infinitely great distances beyond A and beyond B. It will simplify considerations and formulas if we take SB at a node (or place where the depth is equal to b, the mean depth), and we therefore take it so; although this is not necessary for the following kinematic and dynamical statements:—

I. The volumes of fluid crossing SA and SB in the same or equal times are equal; or, in symbols,

$$au = bv = M \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where M denotes the volume of water passing per unit of time.

II. The excess (positive or negative) of the work done by p on any volume of the water entering across SA, above the work done by q on an equal volume of the water passing away across SB is equal to the excess of the energy, potential and kinetic, of the water passing away above that of the water entering. Hence, and by (1), taking the volume of water

unity, we have

$$p - q = \tfrac{1}{2}(v^2 + gb) + \frac{w}{b} - \left[\tfrac{1}{2}u^2 + g(f + \tfrac{1}{2}a)\right] \quad . \quad . \quad (3).$$

Now, calling the pressure at the free surface zero, we have

$$p = \tfrac{1}{2}ga ; \text{ and } q = \tfrac{1}{2}gb + \frac{w'}{b} \quad . \quad . \quad . \quad (4);$$

w' denoting a quantity depending on wave-disturbance. Hence, and by (2),

$$\tfrac{1}{2}M^2 \frac{a^2 - b^2}{a^2 b^2} - g(a - b + f) + \frac{w - w'}{b} = 0 \quad . \quad . \quad (5).$$

Now, put

$$\frac{\tfrac{1}{2}(a + b)}{a^2 b^2} = \frac{1}{D^3}; \text{ and } M = VD \quad . \quad . \quad . \quad (6).$$

Thus D will denote a mean depth (intermediate between a and b and approximately equal to their arithmetic mean, when their difference is small in comparison with either) ; and V will denote a corresponding mean velocity of flow (intermediate between u and v, and approximately equal to their arithmetic mean, when their difference is small in comparison with either).

With this notation, (5) gives

$$b - a = \frac{f - \dfrac{w - w'}{gb}}{1 - \dfrac{V^2}{gD}} \quad . \quad . \quad . \quad . \quad (7).$$

If $b - a$ were exactly equal to f, and if there were no beruffle-ment of the water beyond B, the mean level of the water would be the same in the entering and leaving water at great distances on the two sides of AB; but this is not generally the case, and there is a (positive or negative) rise of level, given by the formula

$$y = b - a - f = \frac{\dfrac{V^2}{gD} f + \dfrac{w - w'}{gb}}{1 - \dfrac{V^2}{gD}} \quad . \quad . \quad . \quad (8).$$

Consider now the case of no corrugation (that is to say, of plane free surface and uniform flow) at great distances beyond B. We have $w - w' = 0$; and therefore

$$y = b - a - f = \frac{\dfrac{V^2}{gD} f}{1 - \dfrac{V^2}{gD}} \quad . \quad . \quad . \quad . \quad (9);$$

or, with V^2 replaced by M/D^2,

$$y = b - a - f = \frac{\dfrac{M}{g D^3} f}{1 - \dfrac{M}{g D^3}} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (10),$$

where, as above,

$$D^3 = \frac{a^2 b^2}{\frac{1}{2}(a+b)} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11).$$

The elimination of b and D between these three equations gives y as a function of f. It is clear that the change of level of the bottom may be sufficiently gradual to obviate any of the corrugational effect; and when this is the case, the equation of the free surface will be found from y in terms of f; f being a given function of the horizontal coordinate, x.

If f is everywhere small in comparison with a, D is approximately constant [much more approximately equal to $\frac{1}{2}(a+b)$], and y is approximately in constant proportion to f.

When the flow is so gentle that V is small in comparison with $\sqrt{gD}$, $\dfrac{M^2}{gD^3}$ is a small proper fraction, and y is approximately equal to this fraction of f.

Generally, in every case when $V < \sqrt{gD}$ the upper surface of the water rises, when the bottom falls, and the water falls, when the bottom rises.

On the other hand, when $V > \sqrt{gD}$, the water surface rises convex over every projection of the bottom, and falls concave over hollows of the bottom; and the rise and fall of the water are each greater in amount than the rise and fall of the bottom; so that the water is deeper over elevations of the bottom, and is shallower over depressions of the bottom.

Returning now to the subject of standing waves (or corrugations of the surface) of frictionless water flowing over a horizontal bottom of a canal with vertical sides, I shall not at present enter on the mathematical analysis by which the effect of a given set of inequalities within a limited space AB of the canal's length, in producing such corrugations in the water after passing such inequalities, can be calculated, provided the slopes of the inequalities and of the surface corrugations are everywhere very small fractions of a radian. I hope before long to communicate a paper to the Philosophical Magazine on this subject for publication. I shall only just now make the following remarks :—

1. Any set of inequalities large or small must in general

give rise to stationary corrugations large or small, but perfectly stationary, however large, short of the limit that would produce infinite convex curvature (according to Stokes's theory an obtuse angle of 120°) at any transverse line of the water surface.

2. But in particular cases the water flowing away from the inequalities may be perfectly smooth and horizontal. This is obvious because of the following reasons :—

(i.) If water is flowing over plane bottom with infinitesimal corrugations, an inequality which could produce such corrugations may be placed on the bottom so as either to double those previously existing corrugations of the surface or to annul them.

(ii.) The wave-length (that is to say the length from crest to crest) is a determinate function of the mean depth of the water and of the height of the corrugations above it, and of the volume of water flowing per unit of time. This function is determined graphically in Stokes's theory of finite waves. It is independent of the height, and is given by the well-known formula when the height is infinitesimal.

(iii.) From No. ii. it follows that, as it is always possible to diminish the height of the corrugations by properly adjusted obstacles in the bottom, it is always possible to annul them.

3. The fundamental principle in this mode of considering the subject is that whatever disturbance there may be in a perpetually sustained stream, the motion becomes ultimately steady, all agitations being carried away down stream, because the velocity of propagation, relatively to the water, of waves of less than the critical length, is less than the velocity of flow of the water relatively to the canal.

In Part II., to be published in the November number of the Magazine, the integral horizontal component of fluid pressure on any number of inequalities in the bottom, or bars, will be found from consideration of the work done in generating stationary waves, and the obvious application to the work done by wave-making in towing a boat through a canal will be considered. The definitive investigation of the wave-making effect when the inequalities in the bottom are geometrically defined, to which I have just now referred, will follow ; and I hope to include in Part II., or at all events in Part III. to be published in December, a complete investigation, illustrated by drawings, of the beautiful pattern of waves produced by a ship propelled uniformly through calm deep water.

XLIII. *On the Electrical Resistance of Soft Carbon under Pressure. By* T. C. MENDENHALL.[*]

A PAPER by the writer on "The Influence of Time on the Change in the Resistance of the Carbon Disk of Edison's Tasimeter," was published in this Journal in July 1882 [Phil. Mag. for August, p. 115]. The object of the paper, as its title indicated, was to present the results of some experiments with the carbon disk which appeared to show that, when pressure was applied, the entire diminution of resistance did not take place at once, but that the reduction continued with diminished rapidity through a considerable period of time. At the conclusion of the paper brief reference was made to investigations of the same subject by Mr. Herbert Tomlinson and by Professors Sylvanus P. Thompson and W. F. Barrett.

Only the conclusion reached by some of these physicists was at that time known to the writer, their verdict being that the observed diminution of resistance was really due to the better surface-contact of the electrodes, and not to any actual change in the specific resistance of the carbon itself.

The last paragraph in the paper contains the following :— " Without knowing anything about the nature of these experiments, the writer desires to record his belief that this theory does not entirely account for the facts stated above."

This, certainly not too rash, declaration of belief in a true pressure-effect was the subject of decidedly unfavourable criticism in the columns of one or two European scientific journals ; and in this Journal of December 1882, Professor Sylvanus P. Thompson published an article entitled " Note on the alleged Change in the Resistance of Carbon due to Change of Pressure," which was an exceptionally severe criticism of the previous paper by the writer. In this article Professor Thompson refers to the investigations of Mr. Tomlinson, Prof. Barrett, and himself, and also to experiments made by Professors Naccari and Pagliani and Mr. Conrad W. Cooke, and he declares that, with the exception of Professor Mendenhall, all who have investigated the point are agreed in their verdict "that this alleged effect was due not to any change in the specific resistance of carbon, but to better external contact between the piece or pieces of carbon and the conductors in contact with them." The truth of this statement is the question at issue. It may be well to remark, however, that although Professor Thompson makes this assertion in December, Mr. Tomlinson had shown, nearly a year earlier, in a paper presented to the Royal Society, on the 26th of the previous January, that the

[*] From Silliman's American Journal for September, 1886.

electrical resistance of hard carbon was diminished by pressure. The amount of the diminution is small, however, and he afterwards expresses the opinion that in such instruments as the microphone transmitter, the greater portion of the observed diminution in resistance is due to variation in surface contact.* Mr. Tomlinson's experiments were made with hard carbon, similar in character to that made use of in experiments to be described presently.

In the summer of 1884, the writer communicated to the American Association for the Advancement of Science a brief account of experiments which satisfied him that the opinion which he had previously expressed concerning the nature of the phenomenon was unquestionably correct. Within the past year the subject has been taken up again, and by means of improved methods and instruments all doubts seem to have been removed.

Innumerable experiments made by physicists of many countries have established, beyond question, the fact that the electrical properties of matter are modified by stress and strain. In carbon the effect of pressure is to diminish resistance. For hard carbon this was established by the investigation of Mr. Tomlinson. In compressed lampblack, as seen in Edison's disks, the effect is very great, and that this is for the most part a true pressure-effect is proved, it is believed, by the experiments about to be described.

In the beginning it was desirable to determine, roughly, the magitude of this effect in the case of hard carbon. For this purpose a copper-plated rod, such as is used in the arc lamp, about 12 centim. in length and 1·5 centim. in diameter, was selected and its ends were ground flat at right angles to its axis. The plating was then removed, except that a band about ·5 centim. in width was left near each end of the rod. Two cork rings 1·5 centim. thick were fitted to the rod, after which they were tunnelled out on the inside, and a hole was made in each so that when they were in place over the copper bands, and mercury was poured in, it would flow around the ring tunnel and make a contact with the carbon as satisfactory as could be desired. The ends of the rod were protected by thin plates of vulcanite, and it was placed between the jaws of a vice. The current from a battery of two or three gravity-cells was passed through the rod by plunging wires into the mercury cups formed by the corks. By this arrangement it was possible to apply pressure at the ends of the rod without in any way influencing the contacts through which the current passed.

* 'Nature,' March 16, 1882.

The terminals of a reflecting-galvanometer whose resistance was about 5000 ohms were also introduced into these mercury cups, and enough additional resistance was introduced to make a convenient deflection of the spot of light upon the scale. When all was adjusted and the spot of light was at rest, the pressure was applied by turning the handle of the vice. In every instance the deflection decreased, showing diminished resistance. This effect was not due to the heat produced by compression, as experiment proved that cause to be inadequate. It was found to be necessary to make the carbon rod decidedly warm to the touch in order to lower the resistance by the same amount ; besides the effect was not transient, as would have been the case if it had been due to the change in temperature. It was also found that compression at right angles to the direction of the current produced a similar effect, but less in magnitude. These facts had been already announced by Mr. Tomlinson.

These experiments with hard carbon or with other rigid bodies are comparatively easy, as there is no difficulty in applying the pressure independent of the contact surfaces, so that possible variation of the latter need not be considered. Unfortunately it appears to be quite impossible to secure this arrangement in the examination of soft carbon. It cannot readily be obtained in forms different from the small disk or button in which it originally appeared, and it is so fragile that it requires the most careful manipulation. Under these circumstances, the only thing to do is to secure the best possible surface-contact of the poles to begin with. Perhaps the ideal arrangement would be a disk with its two opposite faces electroplated with copper, through which a contact with mercury can be secured. The electroplating of two opposite faces of a disk of compressed lampblack is a work of extreme difficulty, and so far as known to the writer has not yet been accomplished, although he is greatly indebted to Mr. Edison for a serious and persistent effort to secure this result, none the less appreciated because, owing to the extremely fragile character of the disk, it proved to be unsuccessful.

It was therefore necessary to depend upon the contact of mercury with the surface of the carbon itself. As this was the contact employed by Professor Barrett in the experiment which Professor Thompson considered " crucial," its use can hardly be objected to in this instance.

The arrangements for the test of the soft carbon were as follows :—two glass tubes about 20 centim. in length were bent at one end into a quarter of a circumference, so that when the two were joined and the straight branches of the tube were

in a vertical position the appearance was that of the letter "U," the height being about 15 centim. Near the lower end of each a short tube was sealed in, over which rubber tubing could be passed, and at the lower part of the curve, in each, a platinum wire was passed through and sealed. The ends of the tubes were ground flat, and they were mounted in such a way that while one was fixed in position, the other could be moved toward or away from it in one plane, and so that the ground ends of the curved parts were always exactly opposite to each other. The movable tube was then taken from its place, the ground edge of its curved end was coated with glue, and it was carefully brought down upon the upper surface of a carbon disk which rested in a horizontal plane. The glue causing the disk to adhere to the tube, the latter could then be secured to its sliding stand, ready to move into place. The ground edge of the fixed tube was now coated with glue, after which the movable tube holding the carbon disk was gently moved up until the disk pressed against the end of the other tube, the glue forming the junction. In this way a carbon wall or partition was formed between the two halves of a "U" tube. When the glue had hardened, mercury was introduced on both sides to a height sufficient to entirely cover the faces of the carbon disk. The current was introduced through the platinum electrodes, which plunged into mercury cups on either side.

In some of the earlier experiments variations of pressure were produced by the addition of mercury to the two branches of the tube, but vastly better than this was the method latterly used, in which the pressure of air was substituted for that of mercury. Glass plates were sealed on the open ends of the two upright branches, thus enclosing a space on each side, except at the small side tubes, to which short pieces of rubber tubing were attached. These were joined by means of a T-tube, so that equality of pressure on both faces of the disk was secured.

The circuit consisted of the battery, the disk, and an additional resistance varying from 3 ohms to 10 ohms for purposes of comparison. The electric ends of the disk and of the resistance were joined to a specially arranged key, by means of which either could be connected with the terminals of a reflecting galvanometer whose resistance was about 7000 ohms. By means of the deflections of the needle of this galvanometer, the resistances were compared and variations noted, the arrangement being substantially the same as that previously used in the experiments with hard carbon. A pressure-gauge, sometimes of water, sometimes of mercury, was attached to

the apparatus to indicate variations in pressure, and these variations were generally produced by blowing from the mouth into a rubber tube about two metres in length.   Very many experiments were made, all without exception showing great diminution in the resistance of the disk by increase of pressure ; and it will be sufficient to quote a few of the results.

The disk is sufficiently sensitive to show very slight changes in atmospheric pressure.   On closing the open end of the rubber tube, and slightly pressing any part of it between the thumb and finger, the spot of light instantly moved, showing decrease of resistance.   A pressure measured by 5 millims. of water produced a decided effect.   The resistance of the disk, with its mercury and platinum wire connections, under ordinary conditions was slightly greater than 6 ohms.   A pressure measured by 5 centim. of mercury instantly reduced it to less than 3 ohms.   If the pressure was maintained, a slow fall of resistance continued for a long time, as found in the previous investigation of the subject.   If the initial pressure was small the recovery would be instantaneous on its removal ; but if it was large, so as to greatly reduce the resistance, it was found that the recovery would not be complete on the withdrawal of the pressure, sometimes falling short by as much as ten per cent., after which a slow rise would take place.   This result is not quite in agreement with the statement made in the first paper upon this subject, which was based, however, upon a much less satisfactory series of experiments.

An examination was made of the effect of the strength of the current upon the resistance of the disk.   The weakest current used was a little less than ·001 ampere, and the strongest was about ·37 ampere, so that one was approximately 400 times the other.   Throughout this range no sensible differences in the resistance of the disk was observed, the agreement at the two extremes being within the errors of measurement.   Under all conditions the effect of variations of pressure was the same.

The faces of a soft carbon disk are always smooth and polished ; the surface of hard carbon, on the contrary, is generally more or less rough and irregular.   It would appear, therefore, that, if the reduction of the resistance of soft carbon by increase of pressure is due to better surface-contact, this reduction of resistance should be much more marked with hard than with soft carbon.   Experiments already described showed that the effect of pressure on hard carbon was very small ; so small, in fact, that the pressure of a few centimetres of mercury would hardly produce a sensible effect.

The substitution of a disk of hard carbon for the soft, in the apparatus described, ought to show, then, whether any considerable part of the resistance-variations observed could be attributed to variation of contact between mercury and carbon. A disk of hard carbon similar in dimensions to the soft disk previously employed was accordingly inserted between two similarly arranged tubes. The result of this experiment was to show, as had been anticipated, a small decrease of resistance when the pressure was increased. A pressure of about 7 centim. of mercury reduced the galvanometer-deflection from 36 to 35 divisions of the scale. This indicates a change of less than 3 per cent., resulting from a pressure which with the soft disk lowered the resistance by more than 60 per cent. There can be little doubt that this small reduction is due almost entirely to better surface-contact produced by pressure.

Throughout all of the experiments with soft carbon, it exhibited more or less irregularity in its behaviour. The application of a pressure very largely in excess of the maximum referred to above would sometimes result in a permanent reduction of the resistance of the disk, indicating that a permanent set had taken place. By the exercise of care, however, what may be called the "normal" resistance may be maintained fairly constant for a considerable length of time.

Conclusions.— When carbon is prepared in the form of compressed lampblack, its electrical resistance varies greatly with the pressure to which it is subjected. A small part of this variation is doubtless to be attributed to change in surface-contact between the carbon and the electrodes through which the current is introduced, but by far the larger part (provided any effort is made to secure good surface-contact) is due to a real change in the resistance of the carbon itself. The resistance of carbon in this condition is fluctuating and uncertain to a degree that seems to prevent its use as a factor in any device for the accurate measure of pressure.

XLIV. *On Dew.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the paper by Mr. Charles Tomlinson, F.R.S., in the September number of your Journal, entitled "Further Remarks on Mr. Aitken's *Theory of Dew*," there is little of scientific interest, the points advanced being mostly of a controversial character. As, however, the whole tone of his

remarks is antagonistic to my work, as he candidly admits at the end of his paper, where he says, " As regards his new theory of Dew I think he has gone astray," it is therefore necessary that I reply to his criticisms.

I sincerely trust Mr. Tomlinson does not think that I accuse him of intentionally raising a false contention by entitling his first paper " Remarks on a *New* Theory of Dew." The title, however, indicated the attitude of the writer's mind towards my conclusions, and it could not be left unchallenged, as it struck at the very root of the matter. Mr. Tomlinson thinks it curious that I should in my last letter have so frequently repeated, in different forms, the statement that my results are not contrary to the teaching of Dr. Wells. If he will refer to his " Remarks " he will find that these repeated statements are all replies to the contents of different paragraphs in his own letter.

Mr. Tomlinson attempts to justify the title of his paper by saying that the author of an article on the same subject in ' Chambers's Journal ' used the same words. That we are right because we think with the majority is an argument which is generally supposed to be a monopoly of the political mind ; to the scientific mind, I venture to say, it carries no weight. Besides, in an article in a popular journal like ' Chambers ' the writer may be pardoned for selecting a catching title, and we do not expect from him the scientific accuracy of language we do from a writer in the ' Philosophical Magazine.' Though the writer in ' Chambers's Journal ' gives a misleading title, yet he very fairly states the position. For instance, he says :—

" The essential difference between the old and the new theories is as to the source of the moisture which forms the dew. Instead of being condensed from the air above by the cooled vegetation, Mr. Aitken maintains that it comes from the ground. The author of the original theory admitted that some of the dew might come from below, but affirmed that it must be an exceedingly small proportion. Mr. Aitken's experiments, on the contrary, seem to prove that most, if not the whole, comes from the ground."

From the above it will be seen that, as I have so frequently stated, my results do not touch on the teaching of Dr. Wells. As every one knows, he concerned himself principally with the condensation of the vapour after it is in the air, and but little with its source, as he distinctly states in his Essay that he had no means of investigating this latter point.

At page 271 Mr. Tomlinson says:—"Again the Chambers's article, referring to Wells, says:—'The points of the grass,

small twigs, and all other good radiating surfaces are cooled the
most ; and accordingly we find the dew-drops most abundant
on these bodies ; whilst on metal, or hard stone surfaces, which
are poor radiators, we seldom or never find any dew.' This is
the Wells picture ; the writer now turns to the Aitken pic-
ture. 'A closer observation reveals the fact that these so-called
'dew-drops' are formed at the end of the minute veins of the
leaves and grass, and are not now recognised as dew at all,
but moisture exuded from the interior of the plants them-
selves.' And yet Mr. Aitken is angry with me for calling
his theory *new*, and for asserting that, if true, it will supersede
the labours of previous observers." My critic here shifts his
ground ; when he finds the contents of the first part of my
paper, regarding the rising of vapour at night, does not justify
his title of *New* Theory, he cleverly seems to put me in oppo-
sition to recognized authority on another point. The above
quotations certainly are in opposition to each other, and great
credit is due for the very ingenious manner in which the case
is put. But, unfortunately for the critic, the words are not
the words of Dr. Wells, but are those of the writer in ' Cham-
bers's Journal,' and can scarcely even be said to be founded
on the teaching of Wells ; so that, further than affording Mr.
Tomlinson a little mental gymnastics, his efforts are here
entirely thrown away.

It does seem strange, considering how much is indirectly
attributed to Dr. Wells regarding the deposition of dew on
grass, that his 'Essay' really contains very little that is definite
either about the radiation from grass, compared with that from
other bodies, or about the amount of dew deposited upon it.
Towards the end of the first part of his ' Essay,' where he
gives " the results of some experiments which were made for
the purpose of ascertaining the tendencies of various bodies
to become cold upon exposure to the sky at night," he
says :—" In the observations hitherto given by me on the cold
connected with dew, the temperature of the grass has been
chiefly considered, partly because my first experiments had
been made upon it, and partly from a wish, which arose after-
wards, to compair my own experiments with those of M. Six,
which had been confined to that substance. I found it, how-
ever, very unfit to furnish the means of compairing the degrees
of cold produced at night on the surface of the earth, at dif-
ferent times and places ; as its state on different nights, on
the same parts of the plat I commonly made use of, and in
different parts of the plat on the same nights, was often very
unequal in point of height, thickness, and fineness ; all of which
circumstances influenced the degree of cold produced by it."

Near the end of the second part of his ' Essay ' he refers to the dew on plants, but it is simply for the purpose of refuting the opinion that " the dew found on growing vegetables is the condensed vapour of the very plants on which it appears." He says :—" This seems to be erroneous, for several reasons. (1) Dew forms as copiously upon dead as upon living vegetable substances. (2) The transpired humour of plants will be carried away by the air which passes over them when they are not sufficiently cold to condense the watery vapour contained in it." . . . . . I draw attention to these passages to show that the distinction between true dew and the dew-drop was not recognized at the time Wells wrote his ' Essay.' All his arguments are directed against the opinion that the dew formed over the whole surface of the blades of plants was produced by the condensation of the vapour transpired by the plants themselves, which he pointed out would be a small quantity at night on account of the absence of light. He makes no reference, so far as I am aware, to the dew-drop, which my investigations tend to show is the result of exuded liquid, and not of the transpired vapour to which Wells directed his criticisms; so that my observations are in a different field from those of Dr. Wells.

But even supposing my conclusions regarding the source of the dew-drop to be correct and to be in opposition to recognized authority, still, if we wish to be accurate, we shall not be entitled to call it a new theory of dew, as it is a theory of the dew-drop as distinct from dew.

If Mr. Tomlinson will change his style of criticism, and will explain to us whence came the drops which formed on the plants experimented on when they were isolated in dry air, and all supply of moisture cut off except that which came up through the tissues of the plants ; and if he will show us that we have misinterpreted the teaching of the experiments made by weighing turfs and others, which we have adduced to show that vapour is given off from the ground while dew is forming at night, he will be entitled to be listened to ; but purely literary criticism is a mere jangling of words, and seldom leads to satisfactory conclusions.

I fear Mr. Tomlinson has misunderstood the bearing of my remarks with regard to what takes place in Persia and the African Desert. The impression I wished to convey was, that we cannot conclude from experiments made in this climate as to what takes place in arid regions. I am happy to say I can assure my critic that " the great forces of Nature rule as impartially in Persia and in Africa as in Scotland ;" is it not for this very reason that we cannot conclude from what

takes place in one country what will take place in the other, unless the conditions are alike?

All my experiments show that in our climate dew never falls on the earth—though of course Mr. Tomlinson, if he is consistent, must think otherwise—and dew is only deposited on plants and other bodies not in good heat communication with the ground. But it would be rash from this to conclude that in arid countries, where the air is highly diathermatous, and the ground dry and probably a bad conductor of heat, the surface of the earth is never cooled by radiation below the dew-point. But on this point I repeat, " I wait for further information before forming any opinion as to what takes place in other and unknown conditions." I observe that Mr. Tomlinson does not observe the same caution, as he states distinctly that, after passing through arid regions, "long before the travellers reached any considerable body of water, nocturnal dews were abundant, and they were deposited from the air, *and did not rise out of the ground.*" Now on what does he found this last statement which I have put in italics? The fact that the dews were deposited out of the air in no way proves that vapour did not rise from the ground while the dews were being deposited.

With regard to the bearing of the experiments of the Florentine Academicians, of Robert Boyle, and Le Roi, my critic says, " These early observers proved that the moisture which forms dew and hoar-frost exists in the air, and does not exhale from the ground." It is a self-evident fact that the vapour must be in the air before it condenses on the different surfaces; but, as has been already said in my previous letter, this fact has "no bearing on the subject," as it neither proves nor disproves that vapour " does not exhale from the ground."

Mr. Tomlinson is welcome to any consolation he can get by shielding himself behind the word " abstract " to account for his misconception of the essential conditions of the crucial experiment made by weighing the turfs before and after "dew-fall." As, however, my last letter gave no further information on the subject, and as Mr. Tomlinson seems now to understand the conditions of this test, it may be presumed that the abstract was complete enough on that point and did not give rise to the misunderstanding.

I may say I have carefully considered the observations of Melloni, referred to by Mr. Tomlinson, and do not find anything in them that affect the conclusions I have arrived at. To enter, however, into a detailed examination of his work would extend the limits of this letter to an undue length.

To many this discussion must have appeared extremely

unsatisfactory and unreal, too much attention having been given to purely literary points, to the almost entire exclusion of the realities connected with the phenomena of dew, and the interpretation to be put on them. No one, I am sure, regrets the unfortunate direction the discussion has taken more than I do. It could not, however, be avoided, owing to the direction from which the attack was delivered.

After all this difference of opinion it is a comfort to find one point on which we are agreed, namely " not to write again on this subject ;" and in closing the discussion I wish to thank Mr. Tomlinson for his criticisms, because I am sure his objections will be the objections of many others to my conclusions ; and though I fear I have not succeeded in making a convert of him, I may perhaps have been more successful with those whose ideas are not so defined and stereotyped by frequent writing on the subject. I think, however, I can assure Mr. Tomlinson that when he candidly and calmly considers the result of my investigations, he will not find them so heterodox as he at present seems to think ; nor do I think that my observations will entirely " supersede the labours of previous observers " for whose work he has so much respect, though some of their views will require modification to meet the present state of our knowledge.

<div align="right">Yours truly,
JOHN AITKEN.</div>

Darroch, Falkirk,
September 11, 1886.

XLV. *On a new Standard Sine-Galvanometer.* *By* THOMAS GRAY, *B.Sc., F.R.S.E.*[*]

THE standard galvanometers commonly employed for the determination of currents in absolute measure consist either of one bobbin of large radius, having a groove of relatively small breadth and depth filled with wire, or of two such bobbins mounted with their planes parallel and at a distance apart equal to the radius of either coil. These coils are suitably mounted for use either as sine- or as tangent-galvanometers. The object of making the coils of large radius is twofold—first, in order that it may be possible to measure it with sufficient accuracy, and second, in order that the magnetic field, produced by the current, may be of nearly uniform strength near the centre of the coil. So far as uniformity of field is concerned, the double coil, or Helmholtz arrangement, is all that can be desired ; but it introduces a multiplicity of measurements, one at least of which, namely

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[*] Communicated by the Author.

the distance between the mean planes of the coils, is difficult to make with sufficient accuracy.

The arrangement here proposed, and illustrated in figures 1 and 2, consists of one layer of wire wound on a tube of comparatively small diameter, say 10 centimetres or less, and of great length. The force at the centre of such a tube, produced by unit current, is given by the equation

$$f = \frac{4\pi n l}{\sqrt{r^2 + l^2}},$$

where l is half the length of the coil, r its radius, and n the number of turns per centimetre of its length. When l is very great compared with r the force becomes $4\pi n$, that is to say, it depends wholly upon the number of turns n per unit-length of the coil. The correction for the length of the tube is shown clearly by expanding $4\pi n\left(1 + \frac{r^2}{l^2}\right)^{-\frac{1}{2}}$, which gives

$$f = 4\pi n\left(1 - \frac{1}{2}\frac{r^2}{l^2} + \frac{1}{8}\frac{r^4}{l^4} - \frac{1}{16}\frac{r^6}{l^6} + \&c.\right).$$

When l is ten times the radius, this series becomes

$$f = 4\pi n\left(1 - \frac{1}{200} + \frac{1}{80000} - \&c.\right),$$

which shows that for this moderate length the correction is very small, and is amply obtained by taking in the second term of the series. Any small error in the measurement of the diameter of the coil is thus of little importance, and hence the chief question becomes the degree of accuracy which can be attained in the estimation of n, the number of turns per centimetre. The total number of turns, or nl, can of course be obtained with absolute accuracy, and the length, l, can be easily measured to the tenth of a millimetre, which, on the assumption of uniformity of winding, gives n to one part in 10,000. Providing that there exists no want of uniformity which will affect the average value of n, perfect uniformity is only important near the central part. Now an irregularity to the extent of one hundredth part of n, or one tenth of a millimetre, can be readily detected by simple measurement ; and supposing that this was due to an opening between the windings in the same plane as the needle (or at the most advantageous position), and that there was no compensation due to denser winding near the middle position, the effect would only amount to about one in 1200 of the total force. The determination of the value of n does not, however, depend altogether on the measurements here described ; the operation of winding a

coil uniformly is one to which mechanical appliances of great precision can be readily applied. The wire can, for instance, be laid on by means of a self-feeding lathe with a pitch which is not only perfectly uniform, but which is also known with minute accuracy from the pitch of the feed-screw. Any slight irregularity which might remain can, if it be within the sensibility of the instrument, be detected by moving the needle along the axis by withdrawing the tube T_1 and observing the change of sensibility. The great advantage of the arrangement is the simplicity and possible accuracy of the measurements and the great uniformity of the field all round the central point. The resistance of the coil is, when thin wire is used, somewhat higher for the same sensibility than it is in the ordinary form, but for a standard instrument this is of little importance.
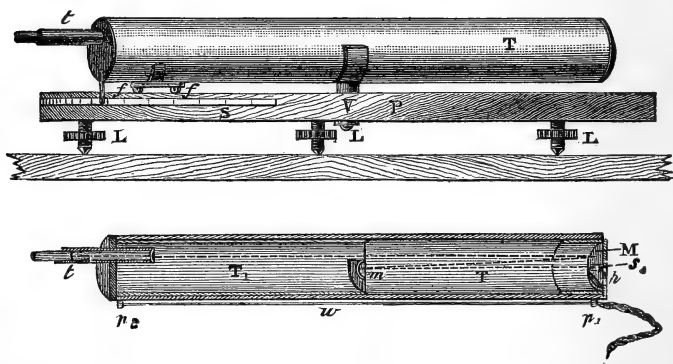
Referring now to the sketches, the tube T, which carries the coil, is mounted on a platform P, furnished with three levelling screws L, L, L, and can be turned round a vertical axis V, sliding at the same time on two feet f, f. The angle through which the coil is turned is measured by a scale S. In the centre of the tube a small plane mirror m is suspended, and at one end of the tube a short scale s, illuminated by means of an inclined mirror or prism, which receives light through a small hole h, is fixed, and immediately above it a plane mirror M. The light from the scale s is reflected from the suspended mirror to the fixed mirror M, and then through the telescope t fixed in the end of the tube T_1. The scale s here shown may be replaced by a narrow slit, or a round hole with a wire across it, and the telescope by a sheet of obscure glass. A lens placed in front of the opening may then be used to focus an image of the slit or wire

on the obscure glass, the position of which can be read on a scale fixed to the glass precisely as in Jacob's well-known galvanometer arrangement.   The lens may be dispensed with, and an ordinary Thomson's spherical mirror substituted for the suspended mirror by fixing the obscured glass in the end of a third tube, which can be telescoped out of and into the tube T_1 so as to adjust the focus.   One end of the coil is attached to the pin p_1 and the other to the pin p_2, while the wire w conducts the current back parallel to the axis of the coil to a third pin p_3, fixed close to p_1.   From p_1 and p_3 the current is conducted, by means of a pair of flexible electrodes twisted together, to proper terminals on the platform P.

In using the instrument, place it on a table in a well-lighted room and level the platform P.   Then turn the coil until the central division of the scale s coincides with the cross wires of the telescope, and take the reading on the scale S.   Pass a steady current through the coil and note how far the tube has to be turned to bring the central division of s again to the cross wire of the telescope.   Repeat this reading with the current reversed, and move the scale S if necessary, until the angles on the two sides of zero are equal.   The current flowing through the coil is then given by the equation

$$C = \frac{H \sin \theta}{4 \pi n \left(1 - \frac{1}{2}\frac{r^2}{l^2}\right)},$$

where θ is the angle through which the tube is turned from the zero position to bring the central division of s to the crosswire of the telescope.   The degree of accuracy attainable in the determination of θ is evidently very great, and can be pushed to almost any extent by fine division of S and microscope reading.

XLVI.   *Problems in Probabilities.*   *By* F. Y. EDGEWORTH, *F.S.S., Lecturer at King's College, London*[*].

SOME interesting problems in the Calculus of Probabilities are presented by the business of Banking[†].   The profits of the Banker depend upon the probability that he will not be called upon to meet at once more than a certain amount of his liabilities.   Assuming that the demands made upon him fluctuate, like so many other phenomena, according to the exponential law of frequency, we may employ the Theory of

Errors to determine the probability that the demand will not exceed any proposed limit. The assumption just made has been defended elsewhere [*]; the mathematical constructions which rest upon that foundation are the subject of this paper.

I. The first and main problem is : Given a series of Banking returns (*e. g.* of Notes in the hands of the Public, or of the Reserve), to find the probability that the next return, or the returns in the proximate future, will not exceed certain limits. The general method is to find the Mean[†] of the given series and the Modulus ; to put T for the ratio of the distance between the Mean and the proposed limit to the Modulus ; to find $\dfrac{2}{\sqrt{\pi}}\displaystyle\int_0^T e^{-t^2}dt$ from the usual tables, and put the value so found for the probability that the next observation on the same side of the Mean as the proposed limit will not exceed the limit, or put half that value for the probability that the next observation unconditionally will not exceed the limit. The question here arises, What method is to be adopted in discovering the Mean and Modulus ? I have elsewhere[‡] considered generally the relative advantages and disadvantages of the different methods. Here it will be sufficient to take account of what is special to the statistics under consideration. It is a peculiarity of Banking returns (as of some meteorological records) that they cannot be regarded as so many independent observations, like the different measurements of the same object, or like the heights of different individuals. Consider, for instance, the weekly returns of Bank-of-England Notes in the hands of the Public between the years 1833 and 1844. Every single return from January to September 1834 is above the Mean of the whole series, which is about £18,400,000. Every single return from August 1839 to June 1841 is below that Mean. Such sequences are inconsistent with the hypothesis of independent observations. We must liken the series of returns, not to a series of numbers each one of which is the sum of, say, twenty digits taken at random (from pages of mathematical tables), but rather to an entangled series constituted in the following manner. Form a first term in the manner just described. Then, for the next term (instead of taking twenty fresh digits), remove one of the constituent digits from the first number and add a fresh digit taken at random. Repeat the process continually. Then you will have an entangled series like the following :—

* Ibid.

† Supposing that there is no secular variation.

‡ "Observations and Statistics," Camb. Phil. Trans. 1885.

95, 99, 99, 92, 95, 94, 94, 99, 101, 100, 108, 108, 106,
105, 103, 103, 107, 112, 105, 105, 101, . . . ;

where the first term and the last term are the only independent observations; and where there occur twenty observations running above the Mean of the series (supposed indefinitely prolonged). In case of such entangled series, I submit that the advantage of accuracy generally attaching to the Arithmetic Mean and the determination of Modulus by way of mean square of error disappears. There is no set-off against their disadvantage of inconvenience. Accordingly the proper mode of reducing such observations is what may be called the Galton-Quetelet method, by way of Median and " Quartile."

I have applied this method to find what was the probability in 1844 that the Notes in the hands of the public would not in the proximate future fall below the limit fixed in that year for uncovered Notes. The returns upon which the calculation is based* are the monthly returns from 1833–1884, and certain biennial returns from 1826–1844. I find that these returns conform pretty accurately to a probability-curve, whose Median is £18,400,000, and whose probable-error is £1,000,000. The Modulus then is about £2,000,000. And the proposed limit is £15,500,000†; that is, at a distance from the Median of about $1\frac{1}{2}$ the Modulus. The probability, then, of the Notes not falling below the regulation-limit in the proximate future is about ·98‡.

II. The problem becomes complicated if we suppose the number of (independent) observations to be not large, and seek to determine, by way of Inverse Probability§, the error due to that limitation.

The error incurred by the Galton-Quetelet method depends upon the errors attaching to the assumptions that the point dividing the given set of observations into two equal groups is the real Median, and that the points dividing the given set of observations in the ratio of three to one are the real " Quartiles." The errors of these assumptions may be investigated by the following method, which was suggested by Laplace's determination of the error incurred by his " Method of Situation " (*Théorie Analytique,* Supplement 2, Sect. 2).

* See 'Mathematical Theory of Banking.'

† £14,000,000+1,500,000 (Bills and Lost Notes), ibid.

‡ More exactly ·98 is the probability of the next observation falling below the limit. Within what time the next observation must take place depends upon the number of (independent) observations assigned to the series. It would be safe, I think, to say—within six months.

§ Problem I. belongs to the class termed d in my 'Observations and Statistics;' Problem II. to $\bar{d}$.

Let n be the number of observations supposed to range under one and the same curve $y = \phi(x)$; whereof P is the (real) ordinate at the Median point; which point may be taken as the origin. The probability of an error z being committed by the apparent Median is equal to the probability of half of the n observations falling on one side and half on the other side of z. Now the probability of a single observation falling outside z is

$$\left(\tfrac{1}{2} - \int_0^z \frac{\phi(z)}{n} dz \right).$$

And the probability of a single observation falling inside z is

$$\left(\tfrac{1}{2} + \int_0^z \frac{\phi(z)}{n} dz \right).$$

Hence the probability of z being the apparent Median is

$$\propto \left(\tfrac{1}{2} - \int_0^z \phi(z) dz \right)^{\frac{n}{2}} \left(\tfrac{1}{2} + \int_0^z \phi(z) dz \right)^{\frac{n}{2}} \propto$$

if z is small

$$\left(\tfrac{1}{2} - \frac{P}{n} z \right)^{\frac{n}{2}} \times \left(\tfrac{1}{2} + \frac{P}{n} z \right)^{\frac{n}{2}} \ (\phi'(0) \text{ being } = 0) \propto \tfrac{1}{4} \left(1 - \frac{2P^2}{n} z^2 \right).$$

The probability of the error z is therefore proportioned to $1 - \dfrac{2P^2}{n} z^2$; or (by a step[*] frequent in this region of mathematics, and which success and the authority of Laplace sanction) $e^{-\frac{2P^2}{n} z^2}$. Now if

$$\phi(x) = \frac{n}{\sqrt{\pi} c} e^{-\frac{x^2}{c^2}}, \quad P = \frac{n}{\sqrt{\pi} c}.$$

And the law of facility for the error of the Median is $\eta = e^{-\frac{2n}{\pi c^2} z^2}$, multiplied by a proper coefficient.

The error incurred by assuming that the point which divides the given observations in two groups numbering respectively $\tfrac{3}{4} n$ and $\tfrac{1}{4} n$ is (the extremity of) the real Quartile, may be similarly calculated. Let Q be the real Quartile, and let us consider the probability of the point $(Q + z)$ dividing the observations in the ratio of $\tfrac{3}{4}$ to $\tfrac{1}{4}$. The probability of an observation falling outside $(Q + z)$ is

$$\left[\tfrac{1}{4} - \left(z \phi(Q) + \frac{z^2}{2} \phi'(Q) \right) \right].$$

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[*] I desiderate a more rigid proof of this step; such as I have attempted to give for the general case of the law of error; "Observations and Statistics," Camb. Phil. Trans. 1885, p. 142.

And the probability of an observation falling inside $(Q + z)$ is

$$\tfrac{3}{4} + z\phi(Q) + \frac{z^2}{2}\phi'(Q)\Big] ;$$

z, as before, being small. Hence the probability of the error z being committed is proportioned to

$$\left[\tfrac{3}{4} + z\frac{\phi(Q)}{n} + \frac{z^2}{2}\frac{\phi'(Q)}{n}\right]^{\tfrac{3}{4}n} \times \left[\tfrac{1}{4} - z\frac{\phi(Q)}{n} - \frac{z^2}{2}\frac{\phi'(Q)}{n}\right]^{\tfrac{1}{4}n}.$$

$$\propto \left[1 + \tfrac{4}{3}z\frac{\phi(Q)}{n} + \tfrac{2}{3}z^2\frac{\phi'(Q)}{n}\right]^{\tfrac{3}{4}n} \times \left[1 - 4z\frac{\phi(Q)}{n} - 2z^2\frac{\phi'(Q)}{n}\right]^{\tfrac{1}{4}n}$$

$$\propto \left[1 - z^2\tfrac{8}{3}\frac{|\phi(Q)|^2}{n}\right] \propto e^{-z^2\tfrac{8}{3}\frac{|\phi(Q)|^2}{n}}.$$

Now $$\phi(Q) = \frac{n}{\sqrt{\pi}c}e^{\frac{-('477c)^2}{c^2}} = \frac{n}{\sqrt{\pi}c}e^{-\cdot227} = \frac{n}{\sqrt{\pi}c}\cdot 8.$$

Whence, as the law of facility for the error of the Quartile point, we have
$$y = Je^{\frac{-1\cdot7n}{\pi c^2}z^2} ;$$

where J is taken so that $\int y\, dz$ between extreme limits $= 1$.

Thus we have found the error incident to both extremities of the line which we assume as the "probable error" of the curve under consideration. And it appears at first sight that we might calculate the error in the length of the line by adding together the Modulus-squared for each of the two errors on which the error of the line depends. There occurs, however, the difficulty that these errors are not independent *. A drift which shifts the Median in either direction is apt to shift the Quartile in the same direction, and *vice versâ*. In view of this difficulty, I see no way but to be contented with the sum of the Moduli-squared as *a superior limit* to the sought Modulus-squared of error; and with the smaller of them as an inferior limit. The superior limit of the Modulus-squared (for the error of the Quartile) is then nearly $\frac{\pi c^2}{n}$. The (superior limit of the) Modulus for the same error is $\sqrt{\frac{\pi}{n}}c$. The error incurred in determining the Modulus (of the given set of observations) follows from the proposition that the Modulus is $2\cdot097 \times$ probable-error. The Modulus for the error in the determination of the Modulus of the given observations is found to be $< 3\cdot7\frac{c}{\sqrt{n}}$; where for c we may put the apparent Modulus (in the example above given, 2,000,000).

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* The same difficulty attends the use of the "Octiles" and "Deciles."

This error may be somewhat reduced, if we determine the probable-error (for the given observations) not by the distance between the apparent Median and the apparent Quartile point, but by the distance between the two Quartile points. The Modulus for the error of c is in this case $\dfrac{1 \cdot 9 c}{\sqrt{n}}$.

We have so far been investigating the error incident to reasoning up to the character of the curve from a not very large number of observations. Let us now consider the error incident to the complete process of reasoning up from the given observations and down again to a new observation. First, let us suppose the Mean given, and calculate the error incident to the assumption that Q', the apparent Quartile, is the real one. Let it be required to determine the probability that a subsequent observation will not exceed $\alpha Q'$. Put $\theta(x)$ for $\dfrac{2}{\sqrt{\pi}} \displaystyle\int_0^x e^{-t^2} dt$. Then, if $Q' + z$ be the real probable error, the probability that an observation being in excess of the Mean will not exceed $\alpha Q'$ is $\theta\left(\dfrac{\alpha Q'}{2 \cdot 097 \, (Q' + z)}\right)$. Now z occurs with a frequency expressed by the facility-curve $y = \dfrac{1}{k\sqrt{\pi}} e^{-\frac{z^2}{k^2}}$, where

$$k = \frac{\sqrt{\pi} c}{1 \cdot 3 \sqrt{n}}$$

nearly. We have therefore as the probability of the next observation above the Mean falling within $\alpha Q'$, the expression

$$\int_{-\infty}^{\infty} \theta \left(\frac{\cdot 477\,\alpha}{1 + \dfrac{z}{Q'}}\right) \frac{1}{\sqrt{\pi} k} \, e^{-\frac{z^2}{k^2}} \, dz.$$

Put $\cdot 477\,\alpha = \beta$, and expand θ in ascending powers of z. The first term of the expanded expression is

$$\theta(\beta) \int_{-\infty}^{\infty} \frac{1}{\sqrt{\pi} k} e^{-\frac{z^2}{k^2}} \, dz = \theta(\beta),$$

the uncorrected value. The second term vanishes. The third term is

$$\frac{1}{\sqrt{\pi}} e^{-\beta^2} (-2\beta^3 + 2\beta) \times \frac{k^2}{Q'^2} \int_{-\infty}^{\infty} \frac{1}{\sqrt{\pi}} \frac{z^2}{k^2} e^{-\frac{z^2}{k^2}} \, d\frac{z}{k}$$

$$= -\frac{1}{\sqrt{\pi}} e^{-\beta^2} (\beta^3 - \beta) \times \frac{k^2}{Q'^2}$$

$$= -e^{-\beta^2} (\beta^3 - \beta) \times \frac{\sqrt{\pi}}{1 \cdot 7 \times \cdot 227 \times n},$$

since

$$k = \sqrt{\frac{\pi}{1 \cdot 7 \, n}} c, \quad \text{and} \quad Q' = \cdot 477 \, c$$

approximately. The odd terms above the third vanish, and the even terms may be neglected, involving the higher powers of k^2, *i.e.* of $\frac{1}{n}$.

In this reasoning we have supposed the Quartile to be determined by taking the point which divides the whole set of given observations into two groups whose numbers are to each other as three to one. If we determined the Quartile by taking the point which divided $\frac{n}{2}$ observations above the Median into two groups each numbering $\frac{n}{4}$, the conclusion would be much the same except that, instead of n, we must put $\frac{n}{2}$, and put $1 \cdot 7$ for 2 in the value of k.

As an example of the last case, let us take $n = 80$; that being, I think, the greatest number of independent observations to which the series of returns of Notes in the hands of the public between 1826–1844 can be regarded as equivalent. And let $\alpha = 4$. Then the correction which is to be made upon the *primâ facie* solution $\theta(4 \times \cdot 477)$ or $\cdot 993$ is $\cdot 008$, or about $\cdot 01$.

This conclusion may be roughly verified by the following table, in which the first column represents several hypotheses as to the relation between Q' the length of the (apparent) Quartile measured from the Mean (supposed given) and c the Modulus of the given observations.

These hypotheses are thus constituted. The central hypothesis corresponds to the case in which a single observation is as likely to fall outside as inside Q'. According to the hypothesis immediately above the central one ($Q' = \cdot 60 \, c$), the probability of a single observation falling inside Q' is $\cdot 6$. According to the hypothesis next above, the corresponding probability is $\cdot 7$. Conversely, below the centre the corresponding probabilities are $\cdot 4$, $\cdot 3$, &c. According to received principles [*] it is allowable to regard each of these hypotheses as *à priori* equally probable.

* See my paper on *à priori* Probabilities (Phil. Mag. 1884, Sept.); also that on "Observations and Statistics" (Camb. Phil. Trans. 1885). It might have seemed more natural, though not really, I think, preferable, to take as the equi-probable hypotheses the equations of Q' to equicrescent values of c (or of c to equicrescent values of Q'). I have done so in constructing analogous tables for the solution of problem iii. (below, p. 381).

Hypotheses as to the ratio of c to Q'.	A posteriori probability of hypothesis × a common factor.	Log Column 2.	Numbers corresponding to column 3 × 10^{12}.	A posteriori probability of each hypothesis.	Ratio of 4Q' to modulus upon each hypothesis.	Conditional probability of deviation greater than 4Q'.	A posteriori probability of subsequent observation exceeding 4Q'.
Q'=·91 c	$(·8)^{20}×(·2)^{20}$	$\bar{16}·081$	·0001	·0000	3·64	·00000	·0000
,, ·74 c	$(·7)^{20}×(·3)^{20}$	$\bar{14}·444$	·028	·016	2·96	·00003	·0000
,, ·60 c	$(·6)^{20}×(·4)^{20}$	$\bar{13}·604$	·40	·23	2·4	·0007	·0002
,, ·477 c	$(·5)^{20}×(·5)^{20}$	$\bar{13}·959$	·91	·51	1·91	·007	·0035
,, ·37 c	$(·4)^{20}×(·6)^{20}$	$\bar{13}·604$	·40	·23	1·48	·0363	·0083
,, ·28 c	$(·3)^{20}×(·7)^{20}$	$\bar{14}·444$	·028	·016	1·12	·1132	·0018
,, ·18 c	$(·2)^{20}×(·8)^{20}$	$\bar{16}·081$	·0001	·0000	·72	·3	·0000
			1·766				·013

The fifth column is obtained by dividing each entry in the fourth column by the sum of them all. The seventh column gives the probability that, if any hypothesis is true, the effect under consideration would follow from it. The eighth column is obtained by multiplying each entry in the seventh column by the corresponding entry in the fifth column. The sum of the entries in the eighth column gives, very roughly of course, the probability that the next observation above the Mean will exceed $4Q'$. The probability that the next observation above the Mean will fall within that limit is ·987, corresponding to ·985 *, as found by the approximative method.

So far we have been supposing the Mean given; but we must now investigate the error incurred in assigning the probability that a subsequent observation will not exceed a certain multiple of the probable-error measured not from the real, but the apparent, Mean. Let O be the real, O' the

apparent Median. Let Q be the real Quartile point, Q' the apparent one, namely, that which separates the given observations into two groups numbering respectively $\frac{3}{4}n$ and $\frac{1}{4}n$. Let $O'P = \alpha O'Q'$. Let us find the probability that the $(n+1)$th observation † will fall outside P.

Let $z = OO'$, $\zeta = QQ'$. And let k be the Modulus for the error of the Median, κ for that of the Quartile point. It is shown above that

$$k = \sqrt{\frac{\pi}{2n}}\, c, \qquad \kappa = \sqrt{\frac{\pi}{1 \cdot 7\, n}}\, c.$$

For any particular values of z and ζ the probability that a subsequent observation will exceed P is proportioned to

$$\tfrac{1}{2}\left[1 - \theta\left(\frac{\mathrm{OP}}{c}\right)\right],$$

where, as before,

$$\theta(x) = \frac{2}{\sqrt{\pi}}\int_0^x e^{-t^2}\, dt.$$

Now $\qquad \mathrm{OP} = \mathrm{OO'} + \mathrm{O'P} = z + \alpha \mathrm{O'Q'}.$

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* The method of quasi-integration by means of a table appears theoretically safer, however practically rough.

† It is a little awkward here to introduce the condition "being above the mean," as in the preceding problems, on account of our uncertainty where the real mean is.

And c (the real Modulus)

$$= 2 \cdot 096 \ldots \times \text{OQ} \text{ (the real probable-error)}$$
$$= 2 \cdot 096 \, (\text{O'Q'} + z - \zeta).$$

The probability of the $(n+1)$th observation exceeding P is therefore proportional to

$$\frac{1}{2} \left[\; 1 - \theta\!\left(\cdot 477 \, \alpha \, \frac{1 + \dfrac{z}{\alpha \text{O'Q'}}}{1 + \dfrac{z - \zeta}{\text{O'Q'}}} \right) \right].$$

This expression is to be multiplied by

$$\frac{1}{\sqrt{\pi}} e^{-\frac{z^2}{k^2}} \times \frac{1}{\sqrt{\pi}} e^{-\frac{\zeta^2}{\kappa^2}} \, dz \, d\zeta,$$

and integrated through the whole range of z and ζ. The first term of the expanded expression is

$$\tfrac{1}{2} \left[1 - \theta(\cdot 477 \, \alpha) \right],$$

which is the uncorrected value of the sought probability. The second term vanishes. For the third term, the correction, I find (putting, as before, β for $\cdot 477 \, \alpha$)

$$e^{-\beta^2} (\beta^3 - \beta) \, \frac{\sqrt{\pi}}{2 \times 1 \cdot 7 \times \cdot 227 \times n} + \frac{e^{-\beta^2} (\beta^3 + 2 \cdot 4\beta) \times \sqrt{\pi}}{\cdot 9n}.$$

The second portion of this addendum is the correction due to the error of putting the apparent Mean for the real Mean. The first part of the expression corresponds to the correction which we obtained when we supposed the real Mean known. The present result differs from the former one in that its sign is negative, and its absolute quantity less by half; as it ought to do in view of the different enunciation of the problem. It should be added that in so far as z and ζ* tend to vary in the same direction, the correction is not accurate but of the nature of a superior limit.

I have applied this correction to the question discussed under Problem I., namely, what was the probability that the limit fixed in 1844 for the uncovered Notes would not be passed in the proximate future by the Notes in the hands of the public. The value found by Problem I., viz. ·98, is reduced by Problem II. to ·95.

III. A third problem is suggested by the procedure of the

* Above, p. 375.

legislators in 1844, when they fixed the limit of uncovered Bank of England notes at £14,000,000; upon the ground that the Notes in the hands of the public (less* by the Bank Post Bills together with the lost notes) had never fallen below that figure. What is the probability that if M is the maximum or minimum (measured from the Mean) of $\frac{n}{2}$ observations, a subsequent observation, the $\left(\frac{n}{2}+1\right)$th in excess (or defect), will not exceed M, or more generally qM. If, like the legislators, we are to ignore *the grouping* of the n observations, and to utilize only the datum that M is the maximum of n observations on one side of the Mean; then *à priori* one ratio of the given maximum to the unknown modulus may be regarded as about as likely as another. Consider the particular ratio r. The probability that a single observation in excess of the Mean will fall within M is $\theta(r)$; where, as before, $\theta(x)$ is identical with $\frac{2}{\sqrt{\pi}}\int_0^x e^{-t^2}dt$. The probability that n observations being in excess of the Mean should not exceed M is $[\theta(r)]^n$. Hence the *à posteriori* probability that the particular hypothesis considered is the true one is

$$[\theta(r)] \div \int_0^\infty [\theta(r)]^n.$$

The probability that if the particular hypothesis is true the next observation above the mean will not exceed qM is $\theta(q$M$)$. Hence the *à posteriori* probability that the next observation will not exceed qM is

$$\int_0^\infty [\theta(r)]^n\, \theta(qr)\, dr \div \int_0^\infty [\theta(r)]^n.$$

I have attempted roughly to evaluate† this expression for certain interesting values of q and n. First, let $\frac{n}{2}=40$ and $q=1$. We have then a problem much the same as that which the legislators in 1844 set to themselves. I find that the odds against the regulation limit £14,000,000‡ being passed in the proximate future were below a hundred to one. A safer limit would have been constructed by putting $q=1\frac{1}{2}$. The odds against this limit (£12,500,000§) being passed are some hundreds to one. Again, suppose $\frac{n}{2}=20$. To obtain the

degree of probability just mentioned it would be necessary to make the limit 2M. These and similar results may be used to find in a rough and ready fashion a superior (or inferior) limit to the fluctuations of any phenomenon; the Mean and the Maximum of a certain number of observations being given. Thus, consider the Registrar-General's returns of the proportions of male to female children for the different counties and for several consecutive years, *e. g.* Report 46, table 16. Omitting Lancashire, West Riding, Huntingdonshire, and Rutlandshire, we have 40 observations of pretty much the same weight in each of the first ten columns in the table referred to. Take any one of these, *e. g.* the fourth, and observe the difference between the maximum return in that column and either its own or the general Mean. The Mean + twice that difference constitutes a fairly safe superior limit. In the case selected, the maximum return is 1089. And the Mean (both of this column and all the columns) is 1038. Hence for the superior limit we have 1140, which is not I think reached by any of the four hundred returns in the table. I have tested the proposed canons in the *Art of Conjecturing* by similarly applying them to various phenomena more or less obeying the law of error; such as[*] a series of figures, each of which is formed by adding together a certain number of digits taken at random from mathematical tables; records[†] of temperature; statistics[‡] of the dactyls in Virgilian lines, &c.

IV. My fourth problem is : Given the Modulus of the law of error for a certain species of demand made upon a bank, to find what the Modulus becomes when the liabilities are increased in any proportion *cæteris paribus.* The general answer is that the Modulus increases, not in the ratio of the volume of business, but in the square root of that ratio. The general rule and the exceptions may be illustrated by examples taken from Vital Statistics. Consider § a Table of Proportions of Male to Female children born in Registration Counties. Form the sums of the columns. Compare the fluctuation of the fringe-row thus formed with the fluctuation of the figures in the ordinary rows. It will be found that the Modulus for the former is not n times, but about $\sqrt{n}$ times,

* See my paper on "Methods of Statistics," Journal of the Statistical Society, Jubilee volume.

† Such as Mr. Glaisher's in 'Philosophical Transactions,' and in the 'British Meteorological Journal'; where the means and the maxima are given ready to hand.

‡ "On Rates," Journal of the Statistical Society, Dec. 1885.

§ See 'Methods of Statistics,' p. 198.

that of the latter. But the general rule is not so well fulfilled when we similarly consider the Death-rates * in Registration Counties. The death-rates in different counties for the same year are not, as the theory requires, independent, but tend to increase or decrease all together. Accordingly the Modulus for the sums does not shrink in the regulation fashion. It will be less indeed than n times, but greater than $\sqrt{n}$ times, the Modulus for the ordinary rows.

A converse † exception may be illustrated by the following supposition. Suppose that, when there is a high death-rate from a particular disease in one year, there is apt to be a low death-rate in the following year or years. Form a table of the rates of deaths from such a disease for a set of counties (like the agricultural) not differing much in respect of healthiness. Consider the fluctuation (not now of the horizontal but) of the vertical fringe of sums in such a table. In virtue of the compensatory action between the years the Modulus for the vertical fringe of sums (or means) will shrink *more* than the general rule requires.

Both these kinds of exception occur in Banking, as will be shown elsewhere ‡. Here it need only be explained that in order to verify the rule, or to establish an exception, there is required a great number of observations. Suppose that C_1^2 is a certain Modulus-squared, and that C_2^2 is another, double C_1^2. In order to prove that relation, it may fairly be required that C_2^2 should be determined so accurately that its error should not exceed $\frac{1}{4}$, or at most $\frac{3}{8}$, C_1^2. In order that the error of C_2^2 should not exceed $\frac{3}{8}$ C_1^2, the Modulus for C_2^2 should not exceed $\frac{3}{16}$ C_1^2. But the Modulus for C_2^2 as determined by n observations is $\dfrac{C_2^2}{\sqrt{n}}$§. Hence $\sqrt{n}$ must be greater $\frac{16}{3}\dfrac{C_2^2}{C_1^2} > 10$. And n must be greater than 100.

The following figures illustrate this theory. Every term in the first series stands for one and the same quantity, a certain Modulus-squared. And similarly every term in the second series stands for another Modulus-squared.

(1) 162, 18, 200, 162, 2, 8, 288, 32, 8, 32, 50, 162, 32, 162, 32, 128, 1250, 1250, 2.........

(2) 32, 200, 8, 128, 338, 8, 32, 578, 648, 32, 32, 200, 8, 128, 338, 8, 32.........

* See my paper " On Methods of ascertaining Rates," Journal of the Statistical Society, Dec. 1885.

† See the discussion of Virgilian statistics in the paper just referred to.

‡ Journal of the Statistical Society, 1886.

§ By Laplace's formula for the error of the mean-square-of-error.

It would be impossible to determine by inspection of these measurements what is the real relation of the objects measured. The Mean of the first series being 210, while that of the second series is 162, those who look only to averages without testing their significance will conclude offhand that the first object is greater than the second. But in fact the real value underlying the first series is only *half* the corresponding value for the second series ; the former being the twice-mean-square-of-error for aggregates of ten digits taken at random, the latter for aggregates of twenty digits. The real relation would come out if we went on long enough. I have gone on long enough, in the case of the first series, to get within a thirty-fifth part of its theoretical value, namely 165. I obtain 160 as the mean of *two hundred and eighty* terms of the first series—a number of observations which corresponds to a probable errror of about 5, the error which I have incurred. If I went on long enough with the second series I should, doubtless, get equally near its real value, which is 330. But to the unitiated such statistics are hopelessly misleading. Blind palpation is sure to err.

XLVII. *Intelligence and Miscellaneous Articles.*

ON THE MEASUREMENT OF VERY HIGH PRESSURES, AND THE COMPRESSIBILITY OF LIQUIDS. BY M. F. AMAGAT.

IN measuring very high pressures I have adopted the principle of the differential manometer ; the conditions to be realized for obtaining exact measurements is that the pistons be completely mobile and at the same time perfectly tight.

M. Marcel Deprez had the idea of dispensing with the leather of the small piston, and making the escape extremely small by a convenient adjustment. This device is insufficient for very high pressures, especially in the conditions of my experiments ; this is also the case with the use of goldbeater's skin, adopted by M. Cailletet.

On the other hand, numerous experiments have shown me that the membrane, on which the large piston rests, introduces several sources of error. I have altogether dispensed with the leather and the membrane, and have solved the difficulty by using a viscous body suitably chosen. The large piston, which only receives the reduced pressure, rests on a cushion of castor-oil, which transmits the pressure to the mercury ; the small piston, which receives all the pressure at the top, becomes quite tight if, after being soaked in oil, and put in its place, it is wetted on its base with a sufficiently viscous liquid, such as molasses, which answers perfectly. In these conditions, the pistons being even somewhat free, there is no real leak, but only an extremely slow oozing, which does not affect the measurements, and this up to pressures higher than 3000 atmospheres.

Nevertheless in these conditions the mercurial column rises with starts which cause considerable errors. They are completely destroyed by giving a rotatory motion to the pistons, which is easily obtained.

I have hitherto investigated only the compressibility of water and that of ordinary ether. The piece in which the piezometer is compressed is a steel cylinder 1·20 metre in height; it is hooped for its entire length except part of the breech; its internal diameter is 3 centim., and its sides are 8 centim. in thickness. It was cast and hooped at the cannon foundry of Firminy. This cylinder is fixed vertically in a large copper reservoir, so that it can be worked with in melting ice, or in a current of water at a constant temperature.

The reading of the volumes of the compressed liquid was made by the following method, which was pointed out to me by Prof. Tait. A series of platinum wires are soldered in the stem of the piezometer, as in fire-alarm thermometers; these wires are connected by a metal spiral with a resistance of two ohms between each wire, and the prolongation of which passes through the short cylinder by means of a special insulated joint. The current of a battery reaches the mercury in which the stem is immersed through the steel cylinder. It will thus be seen how galvanometric indications may give the precise moment at which the mercury rising in the stem, owing to the compression of the liquid, successively reaches each platinum wire.

The liquid of the piezometer and the liquid which transmits the pressure in which it is immersed become considerably heated by the pressure; this makes the experiments very long; a considerable time is required to counterbalance the mass, which is a bad conductor; the readings must be repeated until the indications of the manometer are constant in contact. The series of observations made with decreasing pressures produce the same effect in the opposite direction; the mean of the results is taken, and their general agreement shows that the whole method leaves really little to be desired.

We see by this what gross errors may have been committed with the other devices hitherto used for measuring volumes in analogous conditions.

Ether and water have been studied at zero and at two adjacent temperatures, the one of 20° and the other of 40°.

For both liquids the direction of the variation of the coefficient of contractibility with the temperature is the same under very strong pressures as under very weak. Water continues to form an exception; its compressibility diminishes as the temperature increases, in the above limits; the variation seems, however, to diminish at the highest pressures.

The coefficient of the variation with the pressure, as was easily to be foreseen, gradually diminishes as the pressure increases; and this is the case throughout the entire scale of pressures, contrary

to what is thought by many physicists. This I arrived at in my memoir of 1877, for pressures below 40 atmospheres (*Ann. de Chim. et de Phys.*), and which long before had been found by Colladon and Sturm in their classical work *Sur la Compressibilité des Liquides.*

I shall only give here the results of two series, one on water and the other on ether.

Water at 17°·6.				Ether at 17°·4.			
Pressures, in atmospheres.		Coefficients of compressibility.		Pressures, in atmospheres.		Coefficients of compressibility.
	atm.	atm.			atm.	atm.	
Between	1 and	262	0·0000429	Between	1 and	154	0·000156
,,	262 ,,	805	0·0000379	,,	154 ,,	487	0·000107
,,	805 ,,	1334	0·0000332	,,	487 ,,	870	0·000083
,,	1334 ,,	1784	0·0000302	,,	870 ,,	1243	0·000063
,,	1784 ,,	2202	0·0000276	,,	1243 ,,	1623	0·000057
,,	2202 ,,	2590	0·0000257	,,	1623 ,,	2002	0·000045
,,	2590 ,,	2981	0·0000238				

At 3000 atmospheres the volume of water is reduced by one tenth, and its coefficient of compressibility by one half.

The study of ether will be resumed and carried as far as 3000 atmospheres.—*Comptes Rendus*, August 25, 1886.

ON THE SPECIFIC INDUCTION CONSTANTS OF MAGNETS IN MAGNETIC FIELDS OF DIFFERENT STRENGTHS. BY HILMAR SACK.

Lamont concluded from his experiments on the changes effected by the earth's magnetism on the magnetism of steel bars that the change is greater when the force acts in opposition to the previous magnetization than when it strengthens it. Kohlrausch has recently shown (Wiedemann's *Annalen*, vol. xxii. p. 415, 1884) that such a difference does not exist, at any rate not for a field of the strength of the earth's magnetism.

The object of the present investigation is to ascertain within what limits this equality of the specific induction constants holds. The investigation in question was made at the invitation and under the direction of Prof. Kohlrausch in the Physical Laboratory at Würzburg.

After describing in detail the method of the investigation and the data obtained, the author summarizes the results in the following terms :—

As respects hardened and powerfully magnetized steel bars,—

1. The coefficients of strengthening and of enfeeblement as found by F. Kohlrausch were appreciably the same for fields of the same strength as the horizontal component of the earth's magnetism, that is 0·2.

2. This principle holds also if the magnetic field does not exceed 1·2.

3. If the magnetic field is stronger, then the first coefficient of enfeeblement which is due to the current on opening, first of all exceeds the following ones obtained in the same way by but little, but afterwards by 5 or 6 per cent., if the magnetic field is 3 or 4. Hence in order to obtain a stable value for the strengthening and the enfeeblement with these high values, a bar magnet should first of all be subjected to the same strengthenings and enfeeblements.

4. With powerful magnetic fields the first closing of the current, if this strengthens the magnetic moment of the bar, has a greater constant than the following ones; but it does not attain the magnitude of the constant of enfeeblement produced by the magnetic field on closing.

5. Magnetizing forces, even when they are ten to twelve times as much as the earth's magnetism, produced no considerable durable changes of the permanent state. Forces which were twenty times as strong as the horizontal intensity produced undoubted changes in the moment of the bar.—Wiedemann's *Annalen*, No. 9, 1886.

ON THE ELECTRICAL CONDUCTIVITY OF GASES AND VAPOURS.
BY M. JEAN LUVINI.

It follows from the experiments of Becquerel, Grove, Matteucci, Marangoni, Agostini, and others, that gases and vapours are very bad conductors of electricity. Grove demonstrated this proposition for air at very high pressures; Becquerel and Matteucci for very low pressures (1 to 3 millim.). MM. Mascart and Joubert place air and vapours and generally all gases in the class of bad conductors; and Sir W. Thomson has stated that aqueous vapour is an excellent insulator.

Notwithstanding this we still read in treatises on Physics, and it is repeated in lectures, that moist air and vapours conduct electricity; and this very serious error is the base of several theories.

I have made several sets of experiments on this subject, the results of which, combined with those of other experimenters, have led me to conclude that *gases and vapours, whatever be the pressure and temperatures, are perfect insulators, and cannot be electrified either by friction with each other or with solids or liquids.*

I arrange the experiment so that the fluids in which the electrified bodies are introduced cannot be deposited as liquid on the whole length of the insulating supports. In a large room a long thread consisting of seven cocoon fibres, without torsion and without joints, is stretched. In its centre is suspended a hollow

brass sphere 5 inches in diameter. A second thread, similar and parallel to the first, supports a pith ball pendulum, which, when not electrified, is in contact with the brass sphere.

I usually electrify the sphere with the conducting disk of an electrophorus. The extent of the room and the purity of the air enable me to fill the space about the electrified bodies with a considerable quantity of the gas or vapour which I am investigating, without these fluids settling for a time on the long thread. In this way, apart from the small conductivity of the wires, and the losses due to dust in the air, any diminution of the electrical tension observed must have been due to the conductivity of the fluids experimented on.

I always electrified the sphere almost to the same tension; I then observed the time in which the deflection fell a certain number of degrees, first in air and then by surrounding the sphere and the pendulum by a dense atmosphere of the gas to be studied.

I worked thus with air saturated with aqueous vapour at various temperatures from 16° to 100°; hydrogen and carbonic acid not dried, but as they emerge from the bath in which they are produced; air heated by charcoal or by the flame of a candle, the smoke of an extinguished candle, the fumes of burnt sugar, incense, &c. None of these substances showed any trace of conductivity.

In one set of experiments, instead of cocoon-threads I used ordinary sewing-thread stretched horizontally, and with a double pendulum in the centre provided with pith balls. The results did not change, but when I worked with aqueous vapour at high temperatures, the divergence diminished rapidly by a certain quantity and then became again almost constant.

This effect is due to the vapour which is deposited on the larger and less insulating wire, and to the fact that the small quantity of electricity of the balls becomes divided between the thread and the balls themselves.—*Comptes Rendus,* Sept. 13.

AN ELECTRICAL EXPERIMENT.　BY M. BUSCH.

The author calls attention to the curious figures formed when electricity diffuses on a plate previously dusted with lycopodium powder.

The two balls of a Henley's electrometer are brought in contact with two sides of a dusted glass plate, and a large Leyden jar is discharged through the balls. The figure resulting from the discharge has the appearance of a lightning discharge as seen in the photographs of lightning.—*Beiblätter der Physik,* vol. x. p. 302.

XLVIII. *On the Electrolysis of Silver and of Copper, and the Application of Electrolysis to the Standardizing of Electric Current- and Potential-meters.* By THOMAS GRAY, *B.Sc., F.R.S.E.**

THE following paper contains an account of a large number of experiments on Electrolysis, and on its application to the standardizing of electrical measuring instruments, which have been made during the past year in the Physical Laboratory of Glasgow University: it forms a summary of the reports which have been from time to time made to Sir William Thomson on the subject. The primary object of most of the earlier experiments was to obtain the value, in absolute measure, of the indications of ampere- and volt-meters ; but these experiments were always taken advantage of for the purpose of investigating the reliability of the method under various circumstances as to treatment, size of plates, density of solution, and so on. Many of the later experiments were made specially for the purpose of investigating points of interest suggested by the earlier experiments. It should be borne in mind, with regard to the conclusions put forward in this paper, that they have not been arrived at simply as the result of the experiments made personally by the writer, but that he has had the advantage of seeing the methods applied by several independent experimenters in this laboratory and of comparing the results they obtained.

The electrolysis of copper has for several years been occa-

sionally used by Sir William Thomson as a check on the graduation of galvanometers; but the question of its reliability had not been systematically attacked before the series of experiments here described were taken up. From a number of experiments made in the above laboratory about four years ago, I inferred that copper was capable of giving fairly uniform results; and I obtained as the electrochemical equivalent of copper ·003307, or as the amount of copper deposited by a coulomb of electricity ·0003307. This value has since been found to be too high; and recent experience indicates that there are at least two ways of explaining the discrepance—namely, either that the measurement of the current was in error, or that the solutions were wrongly treated. The first of these needs no remark, it is impossible to tell now whether the current was measured with the needful accuracy or not; but the second will be referred to at some length in this paper. The solutions were in that case very carefully saturated with copper by shaking them up with, and filtering them through, copper oxide; and I bring the matter forward now because I believe that treatment was a serious mistake. It is easy to make a solution of copper sulphate which will give as high, and even a much higher, value for the electrochemical equivalent than that above stated; but once the cause of this is known, it is equally easy to avoid the error. In order to obtain good results it is necessary that the solution be distinctly acid. The ordinary commercial copper sulphate, or the sulphate ordinarily sold as pure, usually contains enough of acid; but the same solution cannot be repeatedly used with safety.

Following the results of the investigations of Kohlrausch, and of Lord Rayleigh and Mrs. Sidgwick, the substances used in the earlier of the present series of experiments were pure silver and pure silver nitrate. Sheet silver was used both for the gain and for the loss plates; it was supplied by Messrs. Johnson and Mathey as pure silver. The form of the cell used for the silver is illustrated in fig. 1 (Plate VII.); it consists of a glass vessel partially filled with a solution of silver nitrate, in which three plates of silver, arranged with their planes parallel, are suspended from spring clips of the form illustrated in figs. 2 and 3. This form of cell offers, in comparison with the platinum-bowl method recommended by Poggendorff and adopted by Lord Rayleigh, some advantages which, in my estimation, outweigh its disadvantages. The total weight of the plate is very small, and hence a light and very delicate balance can be used. The thorough polishing and cleaning of the plates is easier than when a bowl is used. The cleaning of the platinum bowls by dissolving off the

deposit in nitric acid and the satisfactory washing of a bowl generally is troublesome ; indeed the time spent in this operation is often more valuable than if the gain plate, deposit, and all were thrown away ; while, since the deposit is pure silver, the depreciation in value due to cutting up the sheet is small. When the deposit is good it may be rolled, beaten, or burnished down sufficiently to allow the plate to be again used, and thus the operation simply means a transfer of silver from the anode to the cathode. Another and perhaps more important reason for preferring the vertical plates is the fact that, if the plates be properly proportioned as to size and properly prepared, the loss plate can be used with perfect ease as a check on the result of the gain plate. This is a point of some importance in the case of silver, where the deposit is apt to be of such a nature that there is considerable risk of loss in the operation of washing. There is of course the objection to the use of vertical plates, that the density of the solution is apt to decrease at the cathode and increase at the anode. Such an action does take place, and the result is a slightly thicker deposit on the lower part of the plate, thus changing to a small extent the effective area ; but no difficulty has been experienced from this cause, even when the current is allowed to flow for three or four hours, when the current and the solution have the densities stated below.

In the later experiments copper has been almost exclusively used. It is found to be very much more easily managed than silver, and gives, over a wide range of size of plate and density of solution, deposits which are perfectly adherent and homogeneous, thus rendering it more generally convenient where currents of a considerable variety of strength are used. Besides, for large currents such as from 10 to 100 amperes and upwards, the use of silver is almost excluded on account of the expense of the necessary materials. When the highest accuracy is required, however, and when it is used in experienced hands, silver is decidedly superior to copper. It presents hardly any of the uncertainties which, as will be pointed out below, it is absolutely necessary to guard against in the case of copper ; but the fact must not be lost sight of that an expertness in manipulation and a degree of care are required which cannot always be obtained.

The objections which have been brought against the use of copper are its readiness to oxidize in the air, especially if moisture be present, and the fact that it loses weight in the liquid*. Both of these are of course legitimate objections,

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* See a paper by G. Gore, LL.D., F.R.S., 'Nature,' March 16, 1882 ; and Lord Rayleigh and Mrs. Sidgwick, Trans. Roy. Soc. 1884, pp. 411–460.

and more might easily be added ; hence the point to be decided is whether the oxidation and loss in the solution can be avoided or their effects eliminated.   As the result of a good deal of experience, it appears to me that the first objection is of very little weight indeed, as the plate need not be sensibly oxidized in the process of washing and drying if even ordinary care is taken in the operation.   The loss by corrosion in the liquid leads to results which depend to some extent on the size of the plate and the temperature of the solution ; and a great many experiments have been made on the effect this may have on the apparent value of the electrochemical equivalent in different cases.   Examples of such experiments are given below, and they are there discussed more in detail ; but it may be stated here that the effect can be fairly well allowed for if the size of the plate and the strength of the current, as well as the state of the solution, are known.   The error due to this cause need never exceed a tenth per cent., and will generally be considerably less than that.

It appears from experiments on the loss of copper in solutions of sulphate of copper, that a strong solution of normal pure sulphate is at ordinary atmospheric temperature even more active in dissolving copper than the same solution when as much as 5 per cent. of acid has been added ; but that in the use of the normal solution there is a danger that the presence of the copper plates in the cell may render the solution saturated with copper, after which a rapid oxidizing action takes place which invariably causes the gain of weight in an electrolysis experiment to be too great.   The deposit in such a case is of a darker colour than when the solution is kept sufficiently acid ; and indeed a bad plate can generally be detected by simple inspection, but it is well always to add such a quantity of acid (a very little is sufficient) as will prevent any uncertainty on the subject.

Electrolytic Cells.—Some of the arrangements which have been found convenient for the electrolytic cells are illustrated in figs. 1 to 6.   They consist, in the smaller sizes, of round glasses into which three plates, arranged with their planes parallel and about one centimetre apart, are held by means of clips of stiff platinoid or brass wire of the form shown in figs. 2 and 3.   The outer clips are connected together by a cross piece, *a* (fig. 1), at the top, but are insulated from the middle clip by a block of vulcanite, fixed in the cross piece, *b*, into which they are all tightly fitted.   These clips are very convenient, and are easily made by taking a piece of stiff wire, bending it nearly close at its middle point, then winding each half two or three times round a rod of metal of suitable size

so as to form the springs, and after bending the two ends together soldering them to a stiff back-piece, as shown ; care must be taken that the spring front presses firmly on the back before the two are soldered together. Several sets of clips are fitted into one cross piece *b*, so that one frame may serve for one or more cells, the double clips of any one set being connected permanently to the single clip of the next set. When the plates are being placed in the clips, the cross piece which holds them is lifted off the frame and the plates placed in position, the jaws of the clip being opened before the plate is introduced. Care is also taken to open the jaws of the clip before removing the plate, so as to obviate any risk of loss of metal by friction between the holding-points and the plate. This arrangement has been found very convenient for small cells, as it allows the plates to be quickly and almost simultaneously placed in, and removed from, the liquid, and avoids all risk of the plates touching each other or the sides of the vessel ; but for cells to be used with currents of over 10 amperes the plates become rather too large, and the arrangement shown in figs. 4 to 6 is then found more convenient. In this form a frame of insulating material is fitted to the top of the cell, and two sets of spring-contact clips, one set on each of two opposite sides of the cell, are fixed to it. These clips may be of the form just described, or they may be simply flat strips of springy metal soldered to a stiff base piece, as in fig. 5, in such a way that they press firmly against each other. In the use of this form of cell the anode plates are placed in contact with one set of clips and the cathode plates with the other set, the number of anodes being always one greater than the number of cathodes. The form of the plates and the mode of placing them in the cells are illustrated in figs. 4 and 6, from which it will be seen that the surface of the plate above the liquid is made as small as possible by cutting away the plate so as to leave two narrow strips connected to a cross piece, *c, d.* The end *c* of the cross piece is held in the contact clip, while the other end, *d*, is kept in position by a shallow notch in the insulating rim. The insulating frame and attached clips are simply laid on the top of the vessel, and can therefore be lifted off and cleaned so as to insure perfect insulation.

Sizes of Plates and Densities of Solutions.—The size of the plate can be varied within moderate limits in the case of silver, and within very wide limits in the case of copper, without greatly interfering with the quality of the deposit. In the case of silver and silver nitrate the effect of making the plate either too small or too large is, as has been pointed

out by Lord Rayleigh, to make the deposit less adherent and more roughly crystalline. There is then a tendency for the deposit to grow out from the cathode towards the anode in long branch-like crystals. This tendency is increased by any sharp corner on the cathode-plates, and hence care must be taken to round the corners and smooth the edges thoroughly. The deposit also deteriorates as time goes on in consequence of the crystals presenting sharp protuberances, which tend to grow and become more and more prominent. It follows that a somewhat smaller plate or greater density of current can be used if the time be short; but I have generally found that when the density of the current is such that the deposit would deteriorate greatly during the first two or three hours, the adhesion to the plate is likely to be uncertain. The best results seem to be obtained with a solution containing about five per cent. by weight of nitrate of silver, and cathode-plates which present an area of not less than 200 nor more than 600 square centimetres per ampere of current. If this strength of solution and size of plate be used *and the plates be properly cleaned,* the deposit is very compact and finely crystalline, and adheres very firmly to the surface of the plate. When the strength of the solution is increased the size of the plate can be slightly diminished, but not by any means in the proportion of the increased quantity of silver in the solution; the deposit is then more roughly crystalline, will not bear lengthened application of the current, and adheres much less firmly to the plate. So far as these experiments have gone, it seems a mistake to use a solution containing more than, or even as much as, ten per cent. of silver nitrate. Solutions containing from three to thirty per cent. have been repeatedly tried; but the best results have always been obtained with solutions containing from four to ten per cent. There seems no reason for using strong solutions except a slight difference in the original cost of the plates; but as these may be of very thin metal, the cost is a small matter compared with the risk of either total or partial failure of the experiment.

When the loss or anode-plates are to be used simply to supply silver to the solution, they need not be larger than the gain or cathode-plates; and there is some advantage in making them smaller, so as to increase the distance of the edges of the cathodes from those of the anodes. When the plates are small the surface becomes very soft and spongy, and if the density of the current exceed a certain moderate amount they will blacken, and the resistance of the cell is apt to become variable, due to the liberation of gases. It is better to make the anodes a good deal larger than the cathodes, because in

that case the surface of the plate remains bright and moderately hard, so that the plate can be washed and weighed if that be thought necessary. The density of the current at the anode should not exceed one ampere to 400 square centimetres of surface, and should be even less if the plates are to be weighed. The action of the current in causing the solution of the anode is somewhat curious, especially if the plates be made of rolled sheet. If the plates be simply washed and placed in the cell with the bright polished surface still on them, it is seldom that the outside skin will be dissolved. The silver is taken from the interior of the plate, leaving a very thin skin lying loose on the surface, which is ready to fall off either when the plates are lifted out of the cell or when they are placed in water for the purpose of washing. Whether this is due to the mechanical state of the silver on the surface or to a peculiar difficulty in so cleaning the surface of the plate that it is properly wetted by the liquid, is not yet quite clear. A plate of silver when repeatedly used for an anode becomes soft and almost devoid of elasticity, due to the solvent action taking place deep into the interior of the plate ; and it is well to heat the plates to about a red heat in a spirit flame between different experiments, so as to keep it hard and prevent loss of silver.

In the case of copper and copper sulphate, the size of the plate may be almost anything from twenty square centimetres to the ampere upwards; but for experiments which are to be continued for two or three hours, the cathode-plate should present about fifty square centimetres of surface per ampere of current. With plates of from this size upwards the current may be allowed to flow for almost any length of time without introducing the least difficulty as to loss of copper in the subsequent washing. At the higher limit of current-density here mentioned (one fiftieth of an ampere per square centimetre), there is a slight tendency for the deposit to thicken at the edges of the plate and become rough, but as the current-density diminishes this becomes less and less marked. It may be taken as certain that the aggregation of the deposit at the edges of the plate is due to the current-density becoming too great at the sharp edges, and thus causing the formation of sharp crystals, which in their turn intensify the action. If the plates are made large enough, this critical current-density is never reached, and the deposit is as smooth on the edges as elsewhere. It is mainly in the ease with which a perfectly uniform and solid deposit can be obtained that the great advantage of copper over silver for ordinary use in electrolytic measurements lies.

The anodes in the copper cell behave in a similar manner to

that above described for the silver anodes, with the exception that they show no tendency to become soft and inelastic. If the current-density at the anode exceed the fortieth of an ampere per square centimetre, the current is apt to be variable and may almost stop completely, even when an electromotive force of 25 volts is used to produce it[*]. This is due to excessive resistance at the surface of the anode. In some cases the current will, after a few minutes, resume nearly its former strength, and gases are then freely given off at the anode. When the current-density is small, from the two-hundredth of an ampere per square centimetre downwards, the plate can generally be washed, and used to show the loss of copper during the passage of the current. This loss does not in any case agree with the gain on the cathode, in consequence of the solution taking up copper during the experiment; some examples of the results which have been obtained, and some further remarks on this subject are given below. In almost all cases there remains on the surface of the anode more or less of a very fine brown powder, which increases in amount as the current-density is increased, and which also seems to depend somewhat on the nature of the plate. If, for example, a plate of electrotype copper be used, the surface will be found to have become a dark red when the plate is removed from the cell, but no loose copper will be found. That is certainly the case at least so long as the current-density does not exceed the one hundred and fiftieth of an ampere per square centimetre. Currents of greater density were not tried with electrotype copper. This result seems to point either to the mechanical state of the copper in the rolled sheet, or the presence of a somewhat insoluble oxide, as the cause of the loose powder being left on the plate. Local action, due to unequal quality of the plate itself, may have something to do with it; it is certainly greatly increased by frequent reversals of the current in the cell.

The effect of varying the density of the solution of copper sulphate is not great until the density falls to about 1·05, when the deposit begins to be less adherent. There is, however, greater danger of error due to oxidation of the deposit in weak than in strong solutions, when no acid is added, owing to the fact that the solution becomes more quickly saturated with copper. Any density between 1 and 1·18 will be found to answer perfectly, but it is not advisable to use a saturated solution because there is then a risk of crystals forming on the plates.

Preparation of Plates.—In all experiments on electrolysis the proper treatment of the plates previous to their immersion in the liquid is a most important consideration. The treat-

* See note on page 413.

ment which I have found to give the best results with silver is extremely simple. Suppose a new sheet of silver is to be used either for a cathode or anode—first make the corners and the edges round and smooth, and then polish the surface thoroughly with a soft clean pad, water, and fine silver sand, so as to remove the skin which has been in contact with the rolls in the manufacture ; rinse the plate in clean water, or by holding it in a rapid stream of clean water from a water-tap, so as to remove the sand, and then wash it first with clean soap and water and afterwards with clean water ; next place it for a few minutes in a boiling solution of cyanide of potassium, and after that wash it thoroughly in a stream of clean water, taking care not to touch the part of the surface which is to receive the deposit with anything. If the surface of the plate be touched with the fingers, even when they appear to be clean, the markings of the skin will be reproduced by the deposit leaving the parts of the plate bare which were in contact with it. The plate may be dried either in a current of dry air in front of a bright fire, or in any other convenient manner which will insure that the surface of the plate will remain clean, and then accurately weighed. One point about weighing should be mentioned, because it is apt to be over-looked, that is, that the plate must be allowed to assume the temperature of the air before it is weighed, as even a very slight difference of temperature between the plate and the air inside the case of the balance is sufficient to produce quite a sensible error in the weight.

For the preparation of copper-plates a very good plan, especially with large plates and powerful currents, where it is necessary to arrange the resistance of the circuit carefully when the electrolytic cell is included, is, after the edges and corners of the plates have been well smoothed and rounded, to proceed as follows :—Polish the plate thoroughly with silver sand, wash the sand off by holding the plate in a rapid stream of water and rubbing it with a brush or a piece of clean cloth ; place the plate in the cell, as a trial plate, and deposit a thin coating of copper over it, at the same time adjusting the current to the proper strength ; then remove the plate, wash it in clean water, and dry it, first in a clean blotting-pad, and then before the fire, taking care not to heat the plate sensibly. If the plate has not been properly cleaned, the deposit of copper will show any defect; and if it be found to be perfect, the plate may be weighed and the electrolysis continued. For small currents it is more convenient to use a battery of moderately high potential, say twenty or thirty volts, and to keep a resistance in circuit with the cells; and in

this case it is not necessary, after the first trial at least, to adjust the current with the plates in position, as the resistance of the cells can be nearly enough allowed for. In these cases the plates were simply polished either with silver sand and water or with clean sand-paper, then washed in clean water to remove the sand, placed in water slightly acidulated with sulphuric acid for a short time so as to remove any trace of oxide, then rinsed in clean water, and dried first in clean blotting-paper and afterwards in front of the fire or over a spirit-flame. If the plate be thickly oxidized on the surface but otherwise clean, as is often the case with new sheet copper, the quickest mode of cleaning is to wet the plates and then place them for a few seconds in strong nitric acid, rinsing them immediately afterwards in clean water, and putting them into water containing a few drops of sulphuric acid to prevent oxidation. The plates may then be lifted one by one out of the acidulated water, rinsed in clean water, and dried in the manner above described.

The method of drying here recommended is important, as the blotting-pad removes almost at once nearly the whole of the water, and hence the drying can be completed in a few seconds, either in front of a fire or over a spirit-flame, without the slightest oxidation taking place. A plate may be washed and dried in this manner over and over again without producing as much as the tenth of a milligramme of difference in its weight.

Washing the Deposit.—The operation of washing the deposit at the conclusion of the electrolysis is one on the importance of care in which Lord Rayleigh has laid great stress. The method adopted for silver in these experiments was to lift the bar *b* (fig. 1) with the plates attached, and either to dip them several times in clean distilled water contained in a clean glass vessel before removing them from the clips, or to remove them as quickly as possible from the clips, and dip them several times one by one into the water; in either case observing carefully whether any small crystals were removed in the operation. If there is danger of loss of silver from the anodes, it is better to remove the plates from the clips before they are dipped in the water. After this preliminary rinsing to wash off the greater part of the silver nitrate solution, the plates were laid in the bottom of a shallow glass tray containing distilled water, and the water made to flow backwards and forwards over them for a minute or two by raising and lowering one edge of the tray. The plates were then removed and placed in another similar tray containing clean, but not necessarily distilled, water, washed in a similar

manner, and allowed to soak for about fifteen minutes before being dried. It is important to bear in mind that the water used for the first rinsing and washing must be clean water which has been recently distilled, and which has not had an opportunity of absorbing any impurity, such, for example, as a minute quantity of common salt, by coming in contact with anything which has not been recently and thoroughly cleaned. The water supplied by the Glasgow Corporation for domestic purposes, although very nearly pure, clouds immediately on contact with silver nitrate, and throws down a considerable precipitate of silver chloride, no doubt due to the fact that the water contains an appreciable percentage of chlorides in solution. A plate which has been insufficiently washed will generally want brightness when dried, and will assume a somewhat brighter appearance and be found to lose slightly in weight if heated to redness. The comparatively simple washing indicated above will generally be found sufficient, and the plate may be heated without any sensible loss of weight; but it is well not to continue the electrolysis so long as to put a very thick coating of crystals on the plate, as this increases the difficulty in washing and gives no corresponding increase in accuracy from any other source.

The plates were dried by heating the end of the plate in a spirit-flame, the greater part of the water having been previously drained off by holding one corner of the plate in contact with a pad of blotting-paper.

The washing of the copper deposit requires much less care because, if the plate is large enough for the current, it is almost impossible by any ordinary washing to remove copper from the plate. The deposit is solid copper, and may be handled like any other piece of metal without risk of loss. One thing, however, must be attended to, and that is that the plate be not exposed to the air for a longer time than is absolutely necessary before the copper sulphate solution has been completely washed off. The reason for this is that a copper plate oxidizes very rapidly when wet with a solution of nearly neutral copper sulphate and exposed to the air.

The plates should be removed from the solution and at once dipped two or three times into clean water (it need not be distilled), containing two or three drops of sulphuric acid to the litre, and then laid in a tray containing similar water, and washed in the manner above described for the silver plates. The plates may then be lifted out of the acid water, rinsed in clean water containing no acid, and dried, first in a pad of clean white blotting-paper, and afterwards in front of the fire, or, if the plates be small, over a spirit-flame.

No copper will be left on the blotting-paper, and no oxidation will take place if the deposit is of average quality. If the plate has been too small for the current, the drying in the blotting-paper may have to be omitted; and in that case it will be found advantageous to dip the plate in alcohol or ether before drying, as it then dries more quickly and is less likely to be oxidized. The plate should never be raised to a temperature so high that it cannot be held comfortably in the fingers, and evaporation should be, if possible, promoted by holding it in a rapid current of dry air. This latter treatment is that best suited for the loss plates, as they can seldom be placed in a blotting-pad without removing copper; after a little practice, however, it is not difficult to dry without oxidizing them; at the same time it does not seem advisable to make use of the loss plates for ordinary purposes.

Loss of Weight in the Solution.—A number of experiments were made on the effect of leaving silver plates in solutions of silver nitrate, and copper plates in solutions of copper sulphate, for the purpose of finding the rate of loss of weight in these circumstances. The solutions of silver nitrate contained from five to thirty per cent. of the salt, and were in all cases pure with no acid added. The plates were pure silver sheet. There was no appreciable change of weight. The solutions of copper sulphate varied in density from 1·2 to 1·05, and were partly made from pure copper sulphate and distilled water with no acid added, and partly from this solution with from one to twenty per cent. of ordinary commercial sulphuric acid added. The copper plates were in some of the experiments ordinary " high conductivity " sheet copper, and in others thin sheet copper thickly covered with electrotype copper. The results were generally somewhat irregular, but they all showed a considerable loss of copper from the plate to the solution *.

The results of a series of weighings of plates of ordinary copper sheet which were immersed in solutions of copper sulphate of varying density and acidity for a period extending in the aggregate to about seventy-one days are given in

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* Dr. Gore, in the paper above quoted, describes experiments on this subject made by him both on the loss of copper in an acid solution of the sulphate, and on the loss of the anode and cathode plates in a copper-copper-sulphate electrolytic cell. Some of the results there given are in agreement with the results of my experience as given below—for example, the advisability of using high current-density for the measurement of the electrochemical equivalent; but the statement made to the effect that a copper anode loses less by direct chemical action when the current is flowing than when it is not, is not borne out by my experiments.
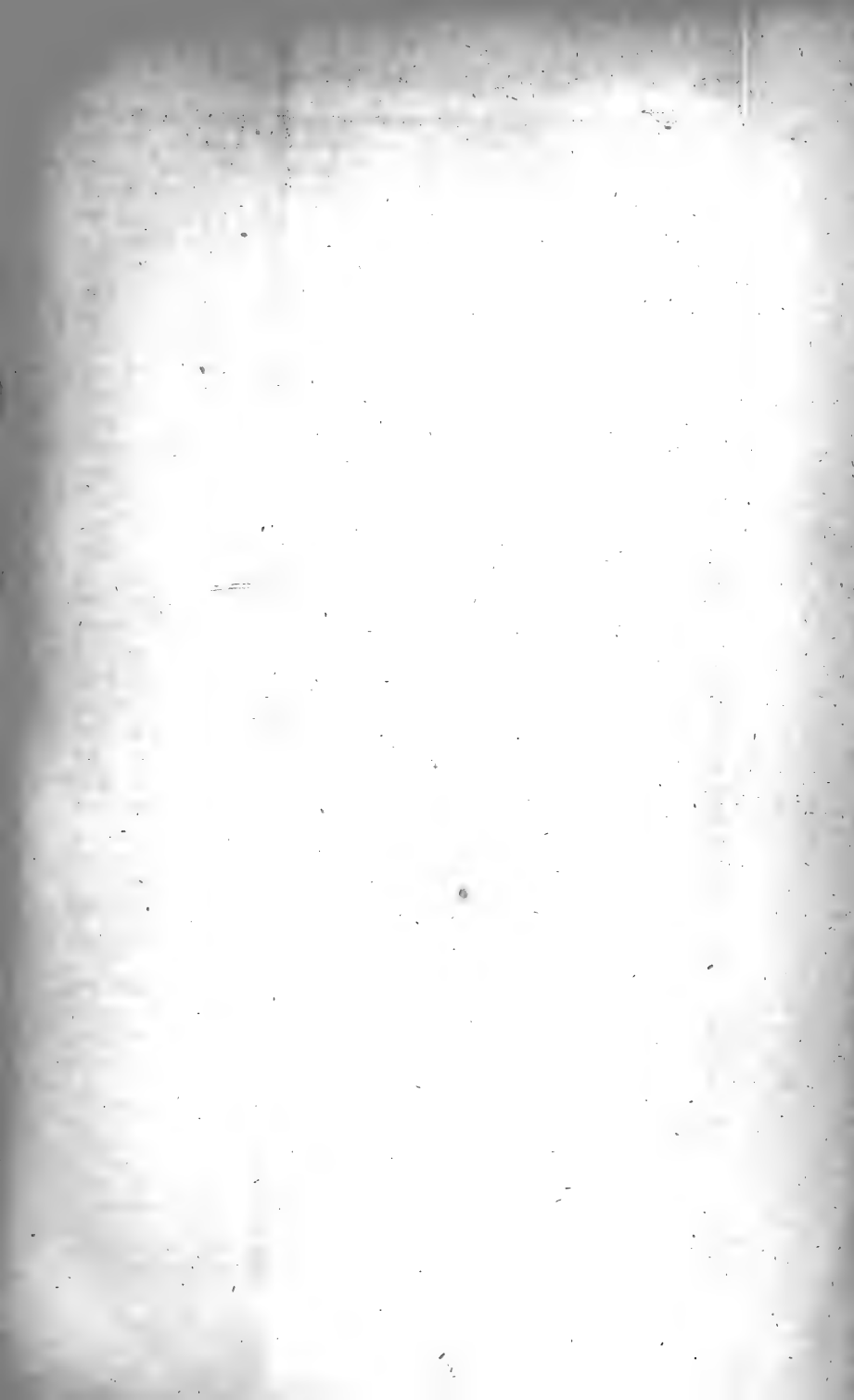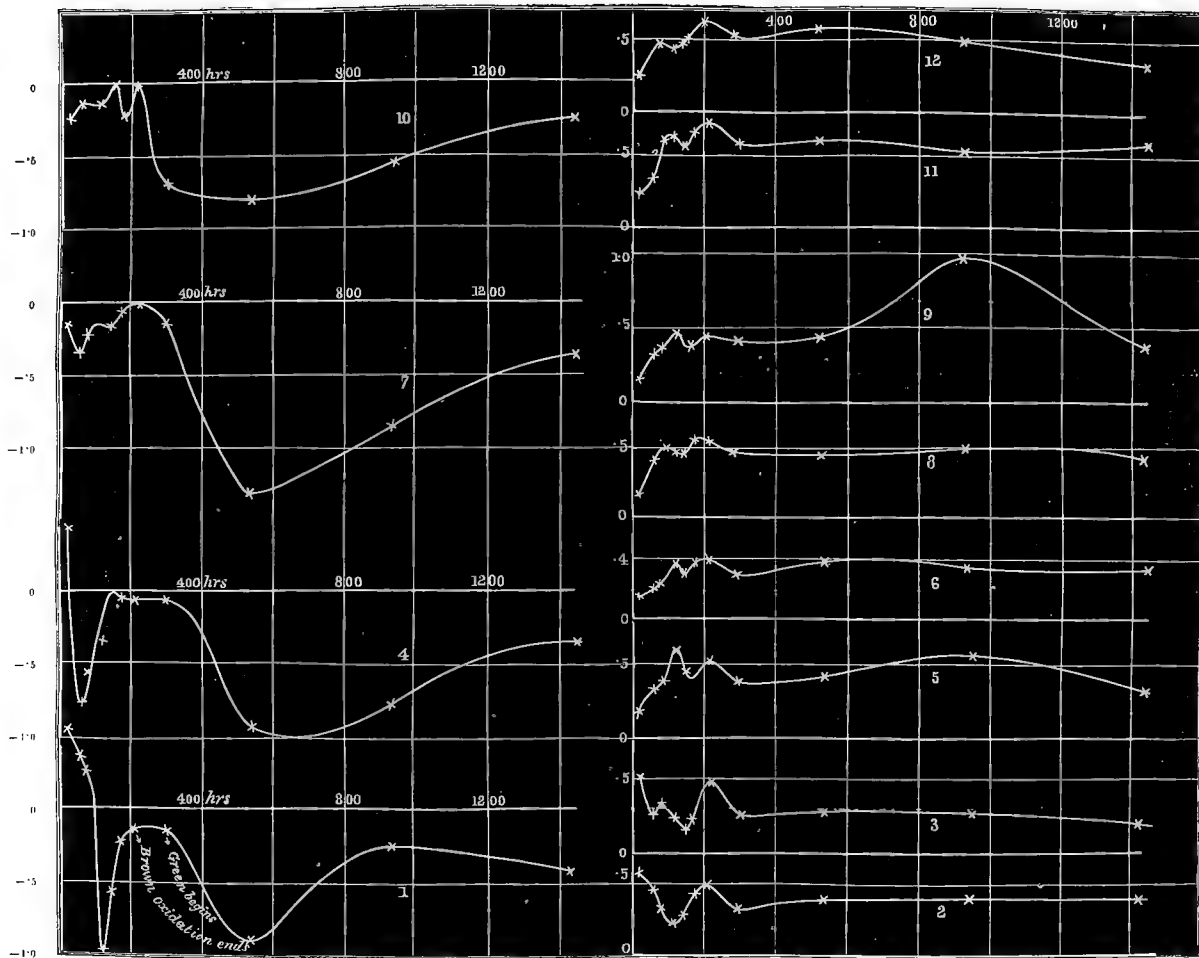
Ordinates are in 100ths of a milligramme, per square centimetre.

Table I., and are illustrated in the curves 1–12. The headings of the Table sufficiently explain the meaning of the different columns. The ordinates of the curves show the rate of loss of copper in hundred thousandths of a gramme per square centimetre of surface per hour. A glance at the Table, or at the curves, is sufficient to show how very variable the loss was, and also that it seldom exceeds the two hundredth of a milligramme per square centimetre per hour. For a plate of fifty square centimetres surface this would amount to a quarter of a milligramme, and the copper which may be deposited on this surface in the same time is, if fifty square centimetres to the ampere be chosen, about 1·2 gramme. Hence if the action be nearly the same during the passage of the current as when no current is passing, the error will amount to about one in three thousand. The actual difference, as will appear from the results discussed below, is greater than this, which seems to indicate a more rapid corrosion of the copper by the liquid when the current is flowing, a result agreeing with those obtained by Gore.

The rate of loss, as shown by the results given in Table I., is at first greater in the dense than in the weak solutions, but seems to reach a minimum between the density 1·10 and 1·15, a result which was confirmed by other experiments, extending over shorter intervals. The loss, however, although never great, is somewhat variable, and it is difficult to make out anything with certainty, unless a very much larger number of experiments are made. The effect of adding acid to the solution seems not to be great when the solution is dense, and seems, for densities between 1·15 and 1·10, rather to retard than to accelerate the action.

The result of subsequent experiments, made with saturated solutions containing different quantities of acid up to twenty per cent., and electrotype copper plates, showed that the effect of the addition of acid on the rate of corrosion was to diminish the rate of loss under these circumstances.

Perhaps the most interesting part of this series of results is that which gives the behaviour of the plates in the solutions to which no acid was added. The first action is a corrosion of the plate, but after a sufficient time has elapsed the corrosion ceases and the plates rapidly gain weight by oxidation. This action goes on until the plates are nearly completely covered with brown oxide, after which the weight remains nearly constant for a few days. Another change then takes place, the plates begin again to increase in weight rapidly, and on examination it is found that they have now begun to form hydrated oxide. This action continues until the plates

TABLE I.—Showing Rate of Loss of Weight of Copper in Solutions of Sulphate of Copper of different Densities, and containing different amounts of free Sulphuric Acid.

For each plate the two sub-columns are:
- **T** = Total loss per square centim., in grammes ÷ 10⁵ ($\div 10^5$)
- **R** = Rate of loss during successive intervals, in grammes per square centim. ÷ 10⁵ ($\div 10^5$)

Initial density →	1·20						1·15						1·10						1·05					
Initial weight →	2·3869		2·7556		2·9777		3·1445		2·8367		2·4508		3·0917		3·1793		2·8144		2·9177		2·6875		2·7225	
Plate No. →	1		2		3		4		5		6		7		8		9		10		11		12	
Date, 1886 / Hours immersed	T	R	T	R	T	R	T	R	T	R	T	R	T	R	T	R	T	R	T	R	T	R	T	R
June 17 (48)	27	0·56	27	0·56	25	0·52	21	·44	9	·19	8·8	·17	−7	−·15	8	·16	8	·16	−12	−·25	9·9	·21	11	·23
June 18 (70)	35	0·36	37	0·45	31	0·27	−4	−·77	16	·32	13	·21	−15	−·36	17	·41	15	·32	−15	−·14	17	·32	13	·09
June 19 (92)	41	0·27	44	0·32	38	0·32	−8	−·55	24	·36	18	·23	−20	−·23	28	·50	23	·36	−18	−·14	13	·59	23	·45
June 20 (140)	−7	−1·00	54	0·21	49	0·24	−25	−·35	44	·63	37	·40	−28	−·16	50	·46	46	·49	−25	−·14	60	·62	53	·42
June 21 (162)	−19	−0·55	60	0·27	53	0·18	−25	−·00	54	·45	44	·32	−32	−·18	60	·45	54	·36	−25	·00	72	·55	63	·45
June 22 (184)	−24	−0·23	69	0·41	58	0·23	−26	−·04	63	·41	52	·36	−34	−·09	72	·55	62	·36	−30	−·23	86	·64	74	·50
June 23 (235)	−31	−0·12	95	0·51	84	0·51	−28	−·04	89	·51	74	·43	−34	−·00	99	·53	84	·45	−32	−·04	121	·72	106	·62
July 3 (375)	−50	−0·13	141	0·33	120	0·26	−36	−·06	140	·59	120	·33	−37	−·14	162	·47	143	·42	−130	−·70	202	·58	178	·55
July 14 (685)	−335	−0·90	257	0·37	200	0·26	−327	−·94	273	·42	244	·40	−450	−·31	297	·44	282	·45	−384	−·82	388	·60	353	·51
Aug. 4 (1189)	−455	−0·24	442	0·37	330	0·26	−710	−·76	550	·55	421	·35	−882	−·86	546	·49	779	·99	−667	−·56	648	·52	598	·57
Aug. 25 (1693)	−665	−0·42	632	0·38	438	0·20	−888	−·35	727	·35	592	·34	−1068	−·37	755	·41	973	·38	−770	−·24	919	·54	760	·49

have become completely coated with a thick green coating, when it gradually ceases. All the phases of this action are illustrated in curves 1 and 4; the first part, namely the loss of weight at the beginning, is not shown in curves 7 and 10, as the plates had already gained weight before the first weighings were made.

Electro-chemical Equivalent of Silver.—Some preliminary experiments were made on this subject, but owing to faults being discovered in the standard galvanometer the experiment was put aside and has not yet been resumed. The first trouble that was experienced with the galvanometer is perhaps worth recording here. It was a new instrument of the sine type, made almost wholly of brass, and had a short needle suspended at the centre of the coil, and a light-index enclosed in a long rectangular box, the sides of which were made of thin brass, and the top and bottom of glass. The coil was forty centimetres in diameter, and consisted of one layer of silk-covered copper wire containing seventy turns, making up a breadth of about five centimetres. The constant of a second and more convenient instrument, of high sensibility, was determined by comparison with the sine galvanometer, and then used in accordance with the method described below for measuring the current passing through two silver-silver-nitrate electrolytic cells arranged in series. The magnetic field at the needle of this auxiliary galvanometer was produced by permanent magnets, and was so intense as to be hardly at all, certainly not to the extent of $\frac{1}{100}$th per cent., influenced by variations of the Earth's magnetism. In the first three trials the constant of the second galvanometer was such that the current produced a deflection of about 20° on the sine galvanometer, an angle which could be measured with a fair amount of accuracy, but which of course did not take full advantage of the sine principle. These three measurements each gave the same result, namely ·0011185 gramme as the amount of silver deposited by a coulomb of electricity. The constant of the second galvanometer was then changed so as to give a deflection of about 40° on the sine galvanometer, and then a difference amounting to about one sixth per cent. was discovered between the value now obtained and that previously got for silver. This seemed to indicate a departure from the law of sines, and suggested, as almost the only possible explanation, magnetic brass. A small Bottomley's reflecting-magnetometer was then set up, and the magnetic field at its needle made almost zero by means of a permanent magnet. The brass box enclosing the needle was unscrewed and, the needle

having been taken out, the box was brought up close to the back of the magnetometer. It was then found to have several magnetic poles, but particularly a north pole on one side and a south pole on the other side of the position where the galvanometer-needle was suspended. This polarity, although sensible enough to the test here made, was so small that it could not be detected with certainty by a magnetometer placed in the Earth's field. The frame of the galvanometer-coil when tested in a similar manner showed signs just perceptible of magnetism, but so small as to produce no sensible effect on the indications of the instrument.

The brass case was then replaced by a glass cell, and two other measurements taken with deflections of about 40° and 60° respectively, the results obtained being ·0011183 and ·0011182; but on continuing the experiment the indications of the galvanometer were found to have become uncertain, due apparently to some defect in the insulation, which has not yet been investigated. The results so far as they go seem to confirm the value given by Kohlrausch and Lord Rayleigh as closely as could be expected, but no great value can be put upon them. In subsequent work the value ·001118 was assumed to be sufficiently accurate for our purpose.

Ratio of Silver to Copper.—A number of experiments were made for the purpose of determining the ratio of the electro-chemical equivalents of silver and copper. These involved the determination of the effect of density of solution, the effect of repeated use of the same solution, and the effect of current-density.

Effect of Density.—The effect of changing the density of the solution was only gone into sufficiently to show that no important error is likely to arise from this cause. The results of two experiments are given in Table II. They show a slightly smaller deposit from weak than from strong solutions; but the absolute difference of weight, only a fifth of a milligramme, if the somewhat doubtful result from the weakest solution be omitted, is too small to found any conclusion upon.

Table II.

Densities.	Comparative weight of deposit in grammes.		Current density in amperes per sq. cm.
	1.	2.	
1·20	·2352	·3519	0·02
1·16	·2350	·3418	0·02
1·12	·2351	·3517	0·02
1·08	·2350	·3518	0·02
1·05	·2347	·3512 (?)	0·02

The number ·3512 has a note against it in my notebook stating that copper was lost in the washing, and generally that the deposits from solutions of this density are unsatisfactory with this size of plate and strength of current. It would appear, therefore, that the last line of the table should be struck out, but, as there is no note against the first result, I have quoted the complete numbers in both cases.

Effect of Acidity.—In the first experiments made on the relative value of the electrochemical equivalents of copper and silver the solution was made with pure sulphate of copper which had been supplied by Burgoyne Burbidges and Co. as very free from acid. The results obtained were at first very puzzling, the gain of weight being generally greater than the loss and very irregular. They are given in Table III., from which the general nature of the result will be readily gathered.

The ratio of the equivalents of copper and silver are not given in the table. They may be calculated from the results given; but as the early results are of no value for this purpose, and the area of the plate has not been recorded for the later ones, it has been thought unnecessary to give them.

The results with the nearly neutral copper sulphate were invariably too high, and this excess of weight was increased by leaving a copper plate in the solution. Repeated use of a solution which has been supersaturated with copper by leaving a copper plate in it seems to bring the solution back towards its normal state.

The addition of a very small percentage of acid causes different solutions to give perfectly accordant results.

One possible explanation of the peculiar action of the neutral solution suggested itself, namely the formation of subsalts of copper when the solution was placed in contact with metallic copper, the subsalt giving twice as much copper as the ordinary sulphate. The real explanation is, however, I believe, the oxidation of the deposit while in the electrolytic cell. In one or two cases the deposit was hammered so as to separate it from the sheet, and it was then found that the back of the deposit obtained with the neutral solution and the surface of the plate were completely oxidized, while those from the acid solution were bright.

Some further experiments were made with various specimens of copper sulphate, some of them obtained in Glasgow and others in London, with the view of finding whether any difference might arise due to a change from one sample of sulphate to another. These need not be quoted in detail, the result being entirely negative. Precisely the same gain of weight was obtained from a solution of the ordinary com-

TABLE III.

Number of experiment.	Silver gain in grammes.	Copper gain in grammes.	Copper loss in grammes.	Remarks.
1.	1·6084	$\left\{\begin{array}{l}·4774\\·4836\end{array}\right.$	·4747 ·4748	
2.	0·8042	$\left\{\begin{array}{l}·2391\\·2373\end{array}\right.$	·2372 ·2372	
3.	1·0059	$\left\{\begin{array}{l}·3053\\·3073\end{array}\right.$	·2967 ·2969	
4.	·9042	$\left\{\begin{array}{l}·2688\\·2728\end{array}\right.$		
5.	1·2063	$\left\{\begin{array}{l}·3589\\·3547\end{array}\right.$	·3496 ·3574	Solution of Nitrate. ,, Sulphate.
6.	1·2041	$\left\{\begin{array}{l}·3546\\·3542\\·3543\end{array}\right.$	·3557 ·3504? } ·3573	Different Sulphate. Same solution as before, with $\frac{1}{10}$ per cent. of sulphuric acid added.
7.	Accidentally lost.	$\left\{\begin{array}{l}·2359\\·2361\\·2360\\·2374\end{array}\right.$	Not weighed } ,, ,, ,,	Ordinary pure sulphate, $\frac{1}{10}$ per cent. acid added. Burgoyne Burbidges as above, $\frac{1}{5}$ per cent. acid added. Ditto, stood for four days with a copper plate in it and no acid added.
8.	1·2067	$\left\{\begin{array}{l}·3550\\·3549\\·3549\\·3549\\·3560\end{array}\right.$	,, ,, ,, ,, ,,	Ordinary pure sulphate, no acid added. Ditto, ditto, with $\frac{1}{10}$ per cent. acid added. Burgoyne Burbidges with $\frac{1}{5}$ per cent. acid added. Ditto, with no acid added.
9.	·8053	$\left\{\begin{array}{l}·2368\\·2368\\·2367\\·2369\\·2371\end{array}\right.$	,, ,, ,, ,, ,,	Solutions the same as last, with the exception of the third, which had 5 per cent. of acid added.

mercial copper sulphate as from the various specimens of pure sulphate experimented on; and for the first few hours no difference was found, whether acid had or had not been added to the solution. If, however, the same solution is to be used for several successive experiments, acid should be added, as will be evident from the results given in Table IV. The first column of this table gives the numbers of the experiments in the order in which they were made ; the second column the ratio of the electrochemical equivalent of copper to that of silver, obtained from the experiments when the current-density at the cathode was one fiftieth of an ampere per square centimetre ; the third column the same ratio when the current-density at the cathode was one two hundred and fortieth of an ampere per square centimetre. Two or more silver cells were always in circuit with the copper cells, but as the differences between them, except in one or two cases where one of them was known not to be reliable because of silver lost previous to weighing, were never so great as to enter into the figures here given, it does not seem necessary to record the numbers in detail.

TABLE IV.

Number of Experiment.	Ratio of the Electrochemical equivalent of Copper to that of Silver.		Remarks.
	Area of plate 50 sq. cms. per ampere.	Area of plate 240 sq. cms. per ampere.	
1.	·2939	·2930	Fresh solutions.
2.	·2944	·2929	Solutions interchanged.
3.	·2941	·2935	Solutions as in 2.
4.	·2942	·2939	,, ,,
5.	·2944	·2942	,, ,,
6.	·2947	·2932	Solutions again interchanged.
7.	{ ·2940 ·2940 ·2940 }	·2934	New solutions once previously used.
8.	{ ·2941 ·2941 ·2940 }	{ ·2930 ·2932 ·2929 }	Fresh solutions containing a little acid.

In all these experiments, with the exception of the 8th, no acid was added to the solution. The quantity of liquid contained in the cells for the experiments 1 to 6 was about 100 cubic centimetres and was the same for the large as for the small plates. The total area of copper plate, including both gain and loss plates, was about 30 square centimetres for the small plates and 60 square centimetres for the large plates.

These experiments show clearly a gradual increase in the ratio with successive experiments, and that in a more marked degree with the small current-density than with the large.

Omitting the results of experiments 2 to 6, the average ratio for the two current-densities here used are ·2940 and ·2931, and the ratio of these two numbers will be found to be in good agreement with the results of special experiments on the effect of size of plate given in Table V. and illustrated in curve 13.

Abscissæ give area of cathode in square centims. per ampere.
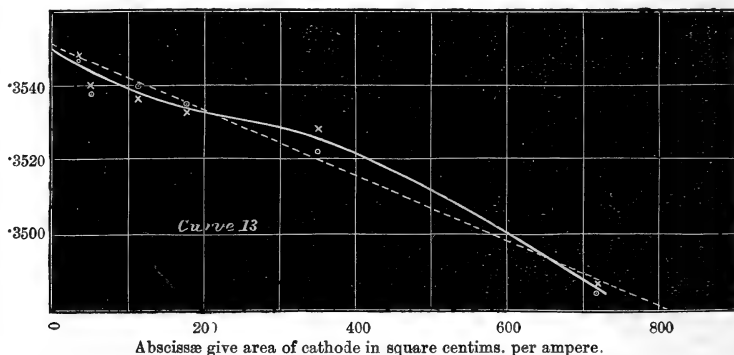
Effect of Size of Plate or Current-Density.—The two experiments on the effect of the density of the current at the cathode, the results of which are given in Table V. and illustrated in curve 13, were made on the 25th and 28th of June respectively; the current, as measured by one of Sir William Thomson's galvanometers, was about ·0997 ampere and was continued for three hours. The first column gives the area of the deposit, the second and third the amount of copper deposited.

TABLE V.

Area of plate in sq. cms.	Weight of deposit in grammes (first experiment).	Weight of deposit in grammes (second expt.).
3	·3534	·3533
5	·3530	·3529
11	·3528	·3530
18·5	·3526	·3527
36	·3524	·3521
73	·3503	·3502

The results given in Table IV. indicate a slightly greater difference between the results obtained with a current-density of a fiftieth of an ampere and those obtained with a two hundred and fortieth of an ampere per square centimetre of

the cathode; but both sets agree in indicating rather less than a tenth per cent. as the correction which has to be added to the results obtained with the denser of these currents, in order to arrive at the true value of the electro-chemical equivalent of copper.

The results of Table IV. give for the amount of copper deposited by a coulomb of electricity, when the cathodes expose a surface of fifty square centimetres to the ampere, ·0003287 gramme, if we assume ·001118 as the corresponding number for silver. They would thus indicate a value not differing much from ·**0003290** as the true value of this constant for copper. In the use of copper for the measurement of currents by electrolysis the absolute value of this latter number is not of much importance; what is wanted is the proper number to use for a certain current-density, and at ordinary temperature this number will not differ much from the number obtained by using ·0003287 for cathodes of fifty square centimetres per ampere, and correcting for other sizes by the dotted line in curve 13.

A few experiments were made with very weak current-densities, the circuit being kept closed for about a week in each case. The results show that when the current-density is very small the rate of loss by direct action of the liquid is much the same as if no current were flowing. The deposit was patchy and did not usually cover all the surface of the plate, a result which was perhaps due to inequalities in the plate itself.

The difference between the gain and loss of weight by the plates in the electrolytic cell is usually very much greater than can be accounted for by the loss of similar plates placed in the same liquid when no current is flowing, a result which appears to be largely contributed to by the anode-plates losing very much more when the current is flowing than when it is not. If the difference between the gain and loss be divided by the sum of the areas of the anodes and cathodes, and the quotient, multiplied by the area of the cathode, be added to the gain, a result is obtained which is always too high to give the true electro-chemical equivalent, but which is very nearly constant for different cells, even when they begin to give uncertain results from the gain of weight taken by itself.

Arrangement of the Circuit.—For experiments with weak currents, such as those the results of which are given in the Tables II. to V., the circuit generally included a battery of twenty-four tray cells, the electrolytic cells, one of Sir William Thomson's improved rheostats, and a galvanometer. The galvanometer was in some of the earlier experiments one of Thomson's lever voltmeters, but in the later experiments one

of his "school galvanometers" arranged so as to have a "false zero," and having its needle suspended in a strong field produced by permanent magnets, was used.   An ordinary index galvanometer when arranged in this way, with its zero well off the scale, gives ample sensibility, and is very compact and convenient.   The current was adjusted by the rheostat until the index was exactly over a division of the scale, and it was kept there by turning the rheostat, if the current varied, all through the experiment.

Standardizing Arrangements. — The arrangement of the apparatus for standardizing instruments intended to measure strong currents such as *deca* and *hecto* ampere-metres is shown in figure 7, which may be taken as a plan of the arrangements on the top of the standardizing table, with the exception that the galvanometer G is, for convenience in the figure, shown much too close to the conductors.   On one end of the table six of the Electric Power Storage Company's accumulators are arranged, and connected by means of thick copper rods to a mercury-cup commutator as shown.   The mercury cups *m* are shown joined across by bridges which are of thick copper in such a way that the cells are in series, but for most purposes these are removed and the cells joined in parallel by means of two rods of copper, provided with teeth at the proper distances apart to fit into the cups *m*, and thus join all in each row together.   The battery, when fully charged and joined in this way, is capable of maintaining a current of two hundred amperes for ten hours. The commutator is joined by means of two copper rods *r, r,* to a distributing board A, by means of which one or more instruments can be put in the same circuit.   In the arrangement shown, a set of conductivity-bridges D and a rheostat R are introduced between the cups 1 and 2 ; a galvanometer G between the cups 4 and 5 ; a pair of large electrolytic cells, joined together by means of a moveable cup M, between the cups 6 and 7 ; and an electric-current balance between 11 and 12.   The arrangement here shown, together with details of the conductivities D and the rheostat R, will be found described by Sir William Thomson in a recent patent specification.   The following description will give a general idea of the apparatus and the mode of using it.

A perspective sketch of two of the conductivity-bridges D is given in figure 8, where one that is in the circuit is shown standing in the mercury troughs *t, t,* and another insulated. These conductivity-bridges consist of U-shaped pieces of platinoid rod or wire according to the conductivity required.   The thick rods are bent into shape and the two limbs held at the proper distance apart by wooden blocks, while the thin wires are

supported all along their length by a strip of wood. The length of rod in each **U** must of course be sufficient for the metal not to become much heated by the potential of cell, one or two volts; the length actually used in the apparatus is four metres. The troughs *t* are partially filled with mercury and have thick copper bottoms, against which the ends of the plantinoid rods press by their own weight. The thin wires have thick terminal pieces fixed to the wooden strips for this purpose. The conductivities of the different bridges, beginning at the end *t*, *t*, fig. 7, are graduated so as to be nearly in the ratio 1, 1, 2, 4, 8, 16, &c.; so that any conductivity between the highest and the lowest can be put in by steps equal to the lowest conductivity in the set. To bridge over these steps and thus make the variation from the highest to the lowest perfectly continuous, the rheostat R, which has a minimum conductivity somewhat less than the lowest of the bridges, is introduced and forms in fact another bridge which can be readily varied. The wire of this rheostat is formed of a strand of fine copper wires, and is capable of carrying a current of ten amperes.

The galvanometer G and the current-balance B may be of any form which it is desired to standardize ; they are simply put into the figure by way of illustration. The electrolytic cells E are rectangular earthenware vessels fitted in the manner illustrated in figs. 4–6, and described on page 392, above. The larger of these cells is fitted to receive plates the aggregate surface of which is sufficient for two hundred amperes, but when the current to be measured is smaller the plates are not all introduced. In this cell the plates are so large that there is considerable danger of their touching each other when they are left freely suspended in the liquid, and hence two **U**'s of thin glass rod are hung over each of the anode plates, so as to keep the cathodes from touching them. The small cell is convenient for currents of from 5 to 25 amperes.

All these operations connected with the treatment of the plates for these cells have been already described, and hence it only remains to indicate how the apparatus is generally used for standardizing instruments. Suppose, for example, a current-balance is to be standardized, and that it is known, by preliminary trials or by comparison with another instrument, to require about fifty amperes to bring its index to the zero mark when a certain weight is put on the beam. Arrange the electrolytic cell so that the cathode plates expose a surface of about twenty-five hundred square centimetres, and join up as shown in the diagram, fig. 7, putting a bridge across from 4 to 5. Introduce conductivity into D until the index of the

balance comes nearly to zero, and then adjust by the rheostat. Leaving everything in position, break the circuit and take out the cathode plates, wash, dry, and weigh them, in the manner already described. Again put the plates in the cell, and observing the time carefully on an accurate time-keeper, close the circuit. The current will at once assume almost the exact strength, and what little deviation there may be can be adjusted in a second or two by the rheostat. From five to ten seconds usually suffices to bring the current accurately to the proper strength, and the electrolysis is as a rule continued for an hour. Now suppose the average error during ten seconds amounts to as much as five per cent., the error on the total amount will only be about one-seventieth per cent., which may be neglected. The current is kept steady by means of the rheostat during the whole hour, and with most instruments this can be done with sufficient accuracy by observing the index of the instrument itself, but if the instrument is not sufficiently sensitive, or if the constancy of its indication is to be tested, a second instrument possessing the requisite sensibility and constancy is introduced into the circuit in the manner indicated at G, and the current kept steady by observing the index of it. The constant of this second instrument does not require to be known ; it should be of such a kind that its constant can be readily changed so as bring its deflection to the proper amount, and the index exactly to a scale-division. The position of the index of the instrument being standardized may then be observed from time to time and the variations, if any, noted.

When instruments which are designed for the measurement either of very weak or very strong currents, as, for example, milliampere- or hectoampere-meters, are to be standardized, it is generally more convenient to send a stronger current in the former case, and a weaker current in the latter case, through the electrolytic cell. Several methods involving the use of auxiliary galvanometers have been used for this purpose ; but the method illustrated in fig. 9 is sufficient to indicate the general principle of divided circuits on which such methods are based.

Referring to the figure, r, r_1 is a set of resistances, supposed in this case to consist of eleven straight wires each of exactly the same resistance and as nearly as possible of the same length and thickness. These wires are soldered to thick bars of copper, b, b_1, b_2, the resistances of which are negligible in comparison with that of the wires. In the case here considered the resistances are supposed to be arranged in two groups of ten and one respectively, but for general purposes it is more

convenient to solder separate terminal pieces to one end of each wire, so that the number in each group may be varied by putting the terminals in one or other of two mercury troughs. Between the bars b_1 and b_2 a zero galvanometer g is connected, and is used for the purpose of indicating when b_1 and b_2 are at the same potential. When this is the case, the current from b to b_1 is to the current between b and b_2 in the ratio of the resistance between b and b_2 to the resistance between b and b_1. Two sets of resistances, or two rheostats, R and R_1, are introduced into the branches for the purpose of adjusting the currents to the required strength. The electrolytic cells E and E_1 are placed in one or other of the branch circuits according as the instrument to be graduated is designed for weak or for strong currents, and the instrument is included in the other branch. The circuit is closed through a suitable battery, which must be of such a kind as will maintain a nearly uniform electromotive force. The current through the instrument G_1 is kept constant by means of the rheostat R_1, and the difference of potential between b_1 and b_2 is kept at zero by means of the rheostat R. When the instrument G_1 is to be standardized by means of a standard galvanometer or a standard current-balance, the arrangement and mode of operation is precisely similar, the standard instrument taking the place of the electrolytic cells.

Note on the Effect of Excessive Current-Density at the Anode in the Electrolysis of Copper Sulphate.—On page 396 above, the effect of excessive current-density at the anode of a copper-copper-sulphate electrolytic cell is referred to, and it is there stated that, unless the anodes present a surface greater than 40 square centimetres per ampere, the current is apt to diminish greatly in strength after the first few minutes.

As the result of some special experiments on this subject I find that the requisite size of anode depends greatly on the degree of saturation of the solution. When the solution is nearly saturated, say above 1·18 in density, the current is apt to be almost entirely stopped on account of the anode plates becoming completely covered by a finely crystalline deposit of copper sulphate which dissolves in the liquid very slowly. A current of one-tenth of an ampere derived from a battery of 24 tray Daniells was passed through an electrolytic cell charged with a solution of sulphate of copper of density 1·18, and having two anode-plates made of round copper wires presenting a total surface of 3·8 square centimetres. The current remained nearly constant for 12 minutes, when it gradually diminished, became practically zero in a few seconds, and remained so for 20 minutes. At the end of this time the

circuit was broken, and the plates taken out of the liquid. The cathode was found to be covered with a good deposit of copper, but the anode was completely enclosed in a glacial sheath of copper sulphate.

Another cell, in which the anode had a surface of 4·4 square centimetres, was treated in the same way, and was found to carry a current of one-tenth of an ampere for 14 minutes. The plates were then found to be covered by a similar coating of copper sulphate.

A similar cell in which the anode had a surface of 7 square centimetres was found to carry one-tenth of an ampere for 30 minutes, when the current fell off as before from the same cause.

Experiments were then made in a similar manner with cells containing a solution the density of which was 1·11. When the current density at the anode exceeded one ampere per 20 square centimetres, the plate became covered with black oxide and the current diminished greatly in strength after a few minutes (about 7 minutes for a current-density of one ampere to 7 square centimetres, and 3 minutes for double that current-density). The current does not entirely cease, and will after a few minutes, if the battery be of sufficiently high potential, again assume nearly its former strength. The oxide then falls off and gases are liberated at the surface of the anode, forming a descending stream close to the plate and an ascending stream two or three millimetres further out. As the current-density is diminished, less and less oxidation takes place and it becomes a lighter brown in colour. With a current-density of one ampere to 30 square centimetres the anode became covered with brown oxide, which made the resistance of the cell high and variable, but little or no gas was generated.

No gas was, so far as could be observed, given off at the cathode during any of these experiments.

XLIX. *On certain Sources of Error in Connection with Experiments on Torsional Vibrations.* By HERBERT TOMLINSON, *B.A.*[*]

Introduction.

DURING a long series of researches on the torsional elasticity and internal friction of metals, I have come across certain sources of error in connection with torsional vibrations

generally, which I venture to bring before the notice of the Physical Society in the hope that by so doing I may perchance save others who may pursue similar investigations from the pitfalls into which I have fallen, and from which I have managed to extricate myself only after a considerable expenditure of time and labour.

In my earlier experiments a wire about 600 centims. in length and 1 millim. in diameter was suspended vertically, having its upper extremity clamped to a rigid support and its lower one clamped or soldered to the centre of a horizontal bar of brass, from which were suspended, by *threads or fine wires*, two cylinders of equal dimensions and mass, and placed at equal distances from the wire. The torsional oscillations of the wire were observed by means of the usual arrangement of mirror, scale, and lamp, the main object of the inquiry being to determine as accurately as possible the logarithmic decrement of the amplitude of swing and the vibration-period. After one set of observations had been completed, the moment of inertia was altered by sliding the cylinders along the bar further away from or nearer to the wire, so that the latter might now vibrate in a different period. If t be the period of vibration, k the moment of inertia, and f the torsional couple,

$$f = \frac{\pi^2 k}{t^2}.$$

The moment of inertia of the bar together with that of the suspended cylinders could be calculated with very fair accuracy; the value of t was also calculated to a nicety; so that it was reasonable to expect that the values of f obtained with different moments of inertia should be very fairly in accordance with each other. For some time this proved to be the case, and several wires of different metals had already been examined when a curious phenomenon presented itself. A change of moment of inertia had just been made, and the wire was then set in torsional oscillation; but instead of the amplitude of swing diminishing by slow degrees as in the previous experiments, the spot of light was seen to make five or six oscillations of very rapidly diminishing amplitude, and finally come nearly to rest. Soon, however, the amplitude began to increase until in a few vibrations it extended over some two hundred divisions of the scale, when once more diminution and, finally, rest nearly ensued. I will not trouble the Society with my conjectures at the time as to the cause of the phenomenon ; suffice it that at length I discovered, what perhaps I ought to have discovered at once, that the, at first sight, startling apparition was due to a very natural cause,

namely, the rotation to and fro of the cylinders about their axes, and that the rapid absorption of energy was owing to the fact that the torsional vibration-period of the wire nearly synchronized with the vibration-period of the cylinders about their axes. I need hardly say that after this discovery I considered it necessary to re-try all my previous experiments with improved arrangements. With more perfect apparatus, in which the cylinders were clamped to the bar so as to be incapable of motion independently of the latter, I was glad to find the main conclusions which I had previously drawn respecting internal friction confirmed. On one occasion, however, by what afterwards turned out to be the merest chance in the world, the old phenomenon appeared with all its curious concomitants, and beats were very plainly discernible between two sets of vibrations which nearly synchronized. These two sets of vibrations I set to work to disentangle in a manner kindly suggested to me by Prof. G. G. Stokes, with whom I had some correspondence on the subject. Suppose that in one or more beat-intervals the slower vibration is dominant*, and afterwards the quicker. If the amplitudes cross during a beat-interval, that one is to be deemed dominant which is dominant about the time of lull. Suppose there are m beat-intervals in which the slower is dominant, followed by n in which the quicker is dominant, and let N be the total number of vibrations counted †. Then the number of vibrations of the two component kinds will be :—

For the slower, N$-2n$,
For the quicker, N$+2m$.

In this way I managed without any difficulty to find that one of the two vibration-periods was nearly a constant whatever the moment of inertia ; whilst the other, that due to the torsional elasticity of the wire, increased as the square root of the moment of inertia. At first I felt very strongly inclined to believe that the former of the two vibration-periods pertained to some molecular state of the wire ; and I was especially deceived by the fact that always after rest the phenomenon became less marked, and, finally, almost vanished, easily, however, to be reproduced in all its former intensity by the slightest shock given to the wire, or by raising or lowering the temperature slightly. The wire was of iron, and my previous experience of the effect of rest on the molecular disposition of this metal led me to draw the above-mentioned conclusion. Had I not been thus deceived, I might more quickly

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* By dominant vibration is meant the vibration of greater amplitude.

† For accuracy, the counting should begin and end with a maximum.

have arrived at the solution of the problem; but as it was four weeks were spent in endeavouring to find the effect of change of temperature, change of load, and change of mode of attaching the cylinders, until I discovered that the phenomenon was simply caused by approach to synchronism between the periods of torsional and *pendulous* vibrations of the wire. Of course, if the axis of the wire passed accurately through the centre of mass of the vibrator, the phenomenon would not occur, but it is impossible *quite* to secure this; and as a consequence so-called centrifugal force on the one hand, and the force of gravity on the other, set up pendulous vibrations which may seriously interfere with the vibration-period due to the torsional elasticity, and still more so with the logarithmic decrement of arc.

Having thus discovered two enemies in ambush, I thought that the ground was now clear, at least as far as any danger from synchronism was concerned; but in this I was mistaken: for happening to pursue my investigations still further with comparatively light loads and small moments of inertia, I at length became acquainted with a third source of error arising from approach to synchronism between the torsional and *transverse* vibrations of the wire. It is, I believe, quite impossible to excite torsional vibrations without at the same time exciting transverse ones; and should synchronism nearly occur between the periods of these vibrations, we may have beats quite as strongly marked as in the two cases before mentioned.

There is yet another source of error to which it is desirable to draw attention. When a wire has been recently suspended, the torsional vibration-period will always be found slightly greater than when it has been suspended for some time and frequently set in vibration. The length of time which must elapse before a wire has reached its state of maximum torsional elasticity depends upon the nature of the metal; with copper, for instance, a few hours will suffice, whilst iron may require at least a couple of days. Now suppose that this state of maximum elasticity has been reached: if we wish to preserve it, care must be taken not to jar the wire or to subject it to a change of temperature of even two or three degrees Centigrade; for in either case the elasticity will be slightly diminished, and a few hundred vibrations will be required to restore it to its former condition. Since it is very difficult to avoid such slight changes of temperature as those mentioned above, it is always advisable after any rest to cause the wire to vibrate a few hundred times before beginning fresh observations.

Determinations of Torsional Electricity.

It is evident, from what has gone before, that when the cylinders are suspended from the bar by threads, the torsional elasticity, *as inferred from the vibration-period,* may be seriously affected should the torsional vibration-period of the wire approach to synchronism with the vibration-period of the cylinders about their axes; and to such an extent did I find this to be the case, that the values of the torsional couple for the same wire, as calculated from the previously given formula, differed from each other by 10 per cent. or more when the moment of inertia was varied.

The effect of synchronism between the torsional vibrations of the wire and the pendulous vibrations is not likely, if proper care be taken in the construction and disposition of the bar and its appendages, to be great; but it may nevertheless be very sensible.

Synchronism between the torsional and transverse vibration-periods is not likely to occur except in extreme cases; but it must be remembered that the torsional vibration-periods may be affected by synchronism between them and *multiples* of the transverse vibration-period, though of course not to the same extent as when the fundamental transverse vibration-period equals the torsional vibration-period. A similar remark to the above would also apply to the other two cases of synchronism.

Determinations of Magnetic Moments.

It is evident that determinations of magnetic moments by the method of oscillations may be similarly affected by the same causes as we have seen would affect determinations of the torsional elasticity.

Damping of Magnets.

Still greater error may be introduced from the above causes in experiments on the damping of vibrating magnets; for the logarithmic decrement is much more seriously affected than the vibration-period.

Determinations of Moments of Inertia.

When it is impossible to determine the moment of inertia of a body by measurement of its dimensions and mass, it is usual to find the time of oscillation, and afterwards to redetermine this time when the moment of inertia has been altered to a known extent.

If t and t' be the two times of oscillation, K the required

moment of inertia, and k the known change of moment of inertia,

$$K = k \frac{t^2}{t'^2 - t^2}.$$

Now a small error in the determination of either t or t' produces a comparatively large error in the value of K; and it is therefore of considerable importance here that there should be no approach to the previously mentioned synchronisms. It is important also here to notice the fourth source of error which has been mentioned; for it is very difficult to change the moment of inertia without imparting some shock to the wire.

L. *On the Self-induction of Wires.*—Part IV. *By* OLIVER HEAVISIDE[*].

A S mentioned at the close of Part III., it would appear that the only practicable way of making a workable system, which will allow us to introduce the terminal conditions that always occur in practice, in the form of linear differential equations connecting C and V, the current and potential difference at the terminals, is to abolish the very small radial component of current in the conductors. This does not involve the abolition of the radial dielectric current which produces the electric displacement, or alter the equation of continuity to which the total current in the wires is subject. The dielectric current, which is $S\dot{V}$ per unit length of line, and which must be physically continuous with the radial current in the conductors at their boundaries, may, when the latter is abolished, be imagined to be joined on to that part of the longitudinal current in the conductors that goes out of existence by some secret method with which we are not concerned.

We assume, therefore, that the propagation of magnetic induction and electric current into the conductors takes place, at any part of the line, as if it were taking place in the same manner at the same moment at all parts (as when the dielectric displacement is ignored, making it only a question of inertia and resistance), instead of its being in different stages of progress at the same moment in different parts of the line. This requires that a small fraction of its length, along which the change in C is insensible, shall be a large multiple of the radius of the wire. The current may be widely different in strength at places distant, say, a mile, and yet the variation in a few yards be so small that this section, so far as the propa-

gation of magnetic induction into it is concerned, may be regarded as independent of the rest of the line ; the variation of the boundary magnetic force or of C fully determining the internal state of the conductors, exactly as it would do were there no electrostatic induction.

In a copper wire, in which $\mu = 1$, and $k = 1/1700$, the value of the quantity $4\pi\mu kp$ is $p/135$. On the other hand, the quantity m in $-s^2 = 4\pi\mu kp + m^2$ has values 0, π/l, $2\pi/l$, &c., or a similar series, in which l is the length of the line in centimetres, so that $j\pi/l$ is a minute fraction, unless j be excessively large. But then it would correspond to an utterly insignificant normal system. We may therefore take

$$-s^2 = 4\pi\mu kp.$$

It will be as well to repeat the system that results, from Part II. The line-integral of the radial electric force across the dielectric being V, from the inner to the outer conductor (concentric tubes), and the line-integral of the magnetic force round the inner conductor being $4\pi C$, so that C is the total current in it, accompanied by an oppositely direct current of equal strength in the outer conductor, V and C are connected by two equations, one of continuity of C, the other the equation of electric force, thus :—

$$-\frac{dC}{dz} = S\dot{V}, \quad e - \frac{dV}{dz} = L_0\dot{C} + R_1''C + R_2''C. \quad (141)$$

Here e is impressed force, S the electrostatic capacity, and L_0 the electromagnetic capacity, or the inductance, of the dielectric, all per unit length of line ; and R_1'' and R_2'' are certain functions of d/dt and constants such that $R_1''C$ and $-R_2''C$ are the longitudinal electric forces of the field at the inner and outer boundaries of the dielectric, which, when only the first differential coefficient dC/dt is counted, become

$$R_1'' = R_1 + L_1\frac{d}{dt}, \quad R_2'' = R_2 + L_2\frac{d}{dt}$$

respectively, where R_1, L_1, and R_2, L_2 are the steady-flow resistance and inductance of the two conductors.

The forms of R_1'' and R_2'' are known when the conductors are concentric circular tubes, of which the inner may be solid, making it an ordinary round wire. Now if the return conductor be a parallel wire or tube externally placed, it is clear that we may regard R_1'' and R_2'' as known in the same manner, provided their distance apart be sufficiently great to make the departure of the distribution of current in them from symmetry insensible. We have merely to remember that it is now the inner boundary of the return tube that corresponds to the

former outer boundary, *i. e.* when it surrounded the inner wire concentrically.

The quantity V will still be the line-integral of the electric force across the dielectric by any path that keeps in one plane perpendicular to the axes of the conductors, in which plane lie the lines of magnetic force. Also, the product VC will still represent the total longitudinal transfer of energy per second in the dielectric at that plane, or, in short, the energy-current. As regards the modified forms of S and L_0, there is, in strictness, some little difficulty, on account of the dielectric being necessarily bounded by other conductors than the pair under consideration, in which others energy is wasted, to a certain extent. This can only be allowed for by the equations of mutual induction of the various conductors, which are not now in question. But if our pair, for instance, be suspended alone at a uniform height above the ground, so that only the very small dissipation of energy in the earth interferes, it would seem, so far as the wire current is concerned, to be an unnecessary refinement to take the earth into consideration. There are, then, two or three practical courses open to us ; as to suppose the earth to be a perfect nonconductor and behave as if it were replaced by air, or to treat it as a perfect conductor. In neither case will there be dissipation of energy except in our looped wires, which have no connection with the earth, but there will be a different estimation of the quantities L_0 and S required. For when we suppose the earth is perfectly conducting, we shut it out from the magnetic field as well as from the electric field. The electrostatic capacity S is that of the condenser formed by the two wires and inter-mediate dielectric, as modified by the presence of the earth (the method of images gives the formula at once), and the value of L_0 is such that $L_0 S = \mu c = v^{-2}$, where v is the velocity of undissipated waves through the dielectric ; that is, as before, L_0 is simply the inductance of the dielectric, per unit length of line. On the ground there will be both electrification and electric current, due to the discontinuity in the electric displacement and the magnetic force respectively ; but with these we have no concern. In the other case, with extension of the magnetic and electric fields, the product $L_0 S$ still equals v^{-2}. Neither course is quite satisfactory ; perhaps it would be best to sacrifice consistency and let the magnetic field extend unimpeded into the earth, considered as nonconducting, with consequently no electric current and waste of energy, whilst, as regards the external electric field, we treat it as a conductor. We must compromise in some way, unless we take the earth into account fully as an ordi-

nary conductor. Similarly, if the line consist of a single wire whose circuit is completed through the earth, by regarding it as infinitely conducting we replace the true variably distributed return-current by a surface-current, and, terminating the magnetic field there, have $L_0 S = v^{-2}$; but if we allow the magnetic field to extend into it, though with insignificant loss of energy by electric current, we shall no longer have this property.

The property is intimately connected with the influence of perfect conductivity on the state of the dielectric. For perfect conductivity will make the lines of electric force normal to the conducting boundaries, will make them cut perpendicularly the magnetic-force lines, which lie in the planes $z =$ const. and are tangential at the boundaries, and will make $L_0 S = v^{-2}$, irrespective of the shape of section of the conductors. Now, at the first moment of putting on an impressed force, wires always behave as if they were infinitely conducting, so that, by the above, the initial effect is simply a dielectric disturbance, travelling along the dielectric, guided by the conductors, with velocity v, irrespective of the form of section. Of course dissipation of energy in the conductors immediately begins, and finally completely alters the state of things, which would be, in the absence of dissipation, the to-and-fro passage of a wave through the dielectric for ever. Except the extension to other than round conductors, this does not add to the knowledge already derived from their study. The effect of alternating currents in tending to become mere surface-currents as the frequency is raised (Part I.) may be derived from, or furnish itself a proof of, the property above mentioned—that at the first moment there is merely a dielectric disturbance. For in rapid alternations of impressed force, we are continually stopping the establishment of the steady state at its very commencement and substituting the establishment of a steady state of the opposite kind, to be itself immediately stopped, and so on.

When the dielectric is unbounded, not enclosed within conductors, there is also the outward propagation of disturbances to be considered; but it would appear, by general reasoning, that this is, relatively to the main effect, or propagation parallel to the wires, a secondary phenomenon.

It is clear that the same principles apply to conductors having other forms of section than circular, when V and C are made the variables, provided the functions R_1'' and R_2'' can be properly determined. The quantity VC being in all cases the energy-current, its rate of decrease as we pass along the line is accounted for (as in Part III.), thus, by making

use of (141), with $e = 0$,

$$-\frac{d}{dz}(\text{VC}) = \frac{d}{dt}(\tfrac{1}{2}\text{SV}^2 + \tfrac{1}{2}\text{L}_0\text{C}^2) + \text{CR}_1''\text{C} + \text{CR}_2''\text{C}\ ;\ (142)$$

that is, in increasing the electric and magnetic energies in the dielectric, and in transfer of energy into the conductors, to the amounts $\text{CR}_1''\text{C}$ and $\text{CR}_2''\text{C}$ per second respectively, which are, in their turn, accounted for by the rate of increase of the magnetic energy, and the dissipativity, or Joule heat per second in the two conductors ; or

$$\text{CR}_1''\text{C} = \text{Q}_1 + \dot{\text{T}}_1, \quad \text{CR}_2''\text{C} = \text{Q}_2 + \dot{\text{T}}_2, \quad . \quad . \quad (143)$$

Q being the dissipativity and T the magnetic energy per unit length of conductor.

These equations (143) must therefore contain the enlarged definition of the meaning of the functions R_1'' and R_2''. For it is no longer true that $\text{R}_1''\text{C}$ is, as it was in the tubular case, the longitudinal electric force at the boundary of the conductor to which R_1'' belongs. It is a sort of mean value of the longitudinal electric force. Thus, we must have

$$\int \text{EH}/4\pi \,.\, ds = \text{CR}_1''\text{C}, \quad . \quad . \quad . \quad . \quad (144)$$

if E be the longitudinal electric force and H the component of the magnetic force along the line of integration, which is the closed curve boundary of the section of the conductor perpendicular to its length. But no extension of the meaning of V is required from that last stated.

Let us, then, assume that R_1'' and R_2'' can be found, their actual discovery being the subject of independent investigation. We can always fall back upon round wires or tubes if required. They are functions of d/dt and constants, if the line is homogeneous. But, as we have got rid of the radial component of current in the conductors, and its difficulties, the constancy of the constants in R_1'' and R_2'' (as the conductivity and the inductivity, or the steady-flow resistance, or the diameter) need no longer be preserved. Provided the conductors may be regarded as homogeneous along any few yards of length, they may be of widely different resistances &c. at places miles apart. Then R_1'', R_2'' become functions of z as well as of d/dt, and S a function of z. Let our system be

$$-\frac{d\text{C}}{dz} = \text{S}''\text{V}, \quad e - \frac{d\text{V}}{dz} = \text{R}''\text{C}, \quad . \quad . \quad . \quad (145)$$

where both R'' and S'' are functions of d/dt and z. As regards S'', it is simply $\text{S}(d/dt)$ when the dielectric is quite non-conducting. But when leakage is allowed for, it becomes

2 G 2

$K + S(d/dt)$, where K is the conductance, or reciprocal of the resistance, of the dielectric across from one conductor to the other. Then both K and S are functions of z. The conduction current is KV, and the displacement current $S\dot{V}$, whilst their sum, or $S''V$, is the true current across the dielectric per unit length of line. We have now, by (145), with $e=0$,

$$-\frac{d}{dz}(VC) = VS''V + CR''C$$

$$= KV^2 + \frac{d}{dt}\tfrac{1}{2}SV^2 + CR''C. \quad . \quad . \quad (146)$$

The additional quantity KV^2 is the dissipativity in the dielectric per unit length, whilst now $CR''C$ includes the whole magnetic energy increase, and the dissipativity (rate of dissipation of energy) in the conductors.

Let V_1, C_1, and V_2, C_2 be two systems satisfying (145) with $e=0$. Then

$$-\frac{d}{dz}V_1C_2 = V_1S''V_2 + C_2R''C_1,$$

$$-\frac{d}{dz}V_2C_1 = V_2S''V_1 + C_1R''C_2 ;$$

from which we see that if the systems be normal, d/dt becoming p_1 and p_2 respectively, we shall have

$$\frac{d}{dz}(V_1C_2 - V_2C_1)$$

$$= (p_1 - p_2)\left\{ SV_1V_2 - \frac{R_1'' - R_2''}{p_1 - p_2}C_1C_2 \right\}, \quad . \quad (147)$$

R_1'' and R_2'' being what R'' becomes with p_1 and p_2 for d/dt. As the quantity in the $\{\,\}$ is the $U_{12} - T_{12}$ of Part III., and the first term is U_{12}, we see that the mutual magnetic energy is

$$T_{12} = C_1C_2(R_1'' - R_2'') \div (p_1 - p_2). \quad . \quad . \quad (148)$$

The division by $p_1 - p_2$ can be effected, and the right member of (148) put in the form

$$C_1f(p_1) \times C_2f(p_2).$$

When this is done, we can find the mutual magnetic energy of any magnetic field (proper to our system) and a normal field, in terms of the total current in the wire and its differential coefficients with respect to t; so that, in the expansion of an arbitrary initial state, C, $\dot{C}$, $\ddot{C}$, &c., may be the data of the magnetic energy, instead of the magnetic field itself.

We see also, from (148), that if T be the magnetic energy of any normal system per unit length of line, then

$$2T = C^2 \frac{dR''}{dp}; \quad \ldots \ldots \quad (149)$$

and therefore, if Q be the dissipativity in the conductors,

$$Q = R''C^2 - \dot{T} = C^2 \left(R'' - p\frac{dR''}{dp} \right). \quad \ldots \quad (150)$$

Now consider the connection of the two solutions for the normal functions. Since the equation of C in general is, by (145),

$$\frac{d}{dz}\left(\frac{1}{S''} \frac{dC}{dz} \right) = R''C - e, \quad \ldots \ldots \quad (151)$$

the normal C function, say w, is to be got from

$$\frac{d}{dz}\left(\frac{1}{S''} \frac{dW}{dz} \right) = R''w, \quad \ldots \ldots \quad (152)$$

with $d/dt = p$ in R'' and S'', making them functions of z and p. Let X and Y be the two solutions, making

$$w = X + qY, \ldots \ldots \ldots \quad (153)$$

where q is a constant. The normal V function, say u, is got from w by the first of (145), giving

$$u = -\frac{1}{S''} \frac{dw}{dz} = -\frac{1}{S''}(X' + qY'), \quad \ldots \quad (154)$$

if $\qquad X' = dX/dz, \qquad Y' = dY/dz.$

In X and Y, which together make up the w in (153), p has the same value. Therefore, in (147), supposing C_1 to be X and C_2 to be Y, we have disappearance of the right member, making

$$\frac{d}{dz}(V_1 C_2 - V_2 C_1) = 0, \quad \text{or} \quad V_1 C_2 - V_2 C_1 = \text{constant},$$

or $\qquad XY' - YX' = S'' \times \text{constant} = hS''$, say, $\ldots$ (155)

leading to the well-known equation

$$Y = X \int \frac{hS''}{X^2} dz,$$

connecting the two solutions of the class of equations (152); which we see expresses the reciprocity of the mutual *activities*

of the two parts into which we may divide the electromagnetic state represented by a single normal solution.

Also, by (147), integrating with respect to z from 0 to l,

$$\int_0^l \mathrm{S}u_1u_2\,dz - \int_0^l \frac{\mathrm{R}''_1 - \mathrm{R}''_2}{p_1 - p_2}\,w_1w_2\,dz = \frac{\left[u_1w_2 - u_2w_1\right]_0^l}{p_1 - p_2}, \quad . \quad (156)$$

either member of which represents the complete $\mathrm{U}_{12} - \mathrm{T}_{12}$ of the line. The negative of this quantity, as in Part III., is the corresponding $\mathrm{U}_{12} - \mathrm{T}_{12}$ in the terminal arrangements; so that the value of $2(\mathrm{U} - \mathrm{T})$ in a complete normal system, including the apparatus, is

$$2(\mathrm{U}-\mathrm{T}) = \int_0^l \mathrm{S}u^2\,dz - \int_0^l \frac{d\mathrm{R}''}{dp}\,w^2\,dz - w_1{}^2\frac{d\mathrm{Z}_1}{dp} + w_0{}^2\frac{d\mathrm{Z}_0}{dp}, \quad . (157)$$

if $\mathrm{V}/\mathrm{C} = \mathrm{Z}_1$ and Z_0 at $z = l$ and 0, these being functions of p and constants, and w_1, w_0 are the values of w at $z = l$ and 0. Or, which is the same,

$$2(\mathrm{U}-\mathrm{T}) = \left[w^2\frac{d}{dp}\left(\frac{u}{w} - \mathrm{Z}\right)\right]_0^l, \quad . \quad . \quad . \quad (158)$$

as before used.

There is naturally some difficulty in expressing the state at time t, thus :—

$$\mathrm{V} = \Sigma \mathrm{A}u\epsilon^{pt}, \qquad \mathrm{C} = \Sigma \mathrm{A}w\epsilon^{pt},$$

due to an arbitrary initial state, on account of the difficulty connected with

$$(\mathrm{R}_1'' - \mathrm{R}_2'') \div (p_1 - p_2),$$

and the unstated form of R''. But when the initial state is such as can be set up by any steadily-acting distribution of longitudinal impressed force (e an arbitrary function of z), so that whilst V is arbitrary, C is only in a very limited sense arbitrary, and $\dot{\mathrm{C}}$, $\ddot{\mathrm{C}}$, &c. are initially zero, and certain definite distributions of electric and magnetic energy in the terminal apparatus are also necessarily involved; in this case we may readily find the full solutions, and therefore also determine the effect of any distribution of e varying anyhow with the time. In fact, by the condenser method of Part III., we shall arrive at the solution (135); we have merely to employ the present u and w, and let M be the value of the right member of (158). The following establishment, however, is quite direct, and less mixed up with physical considerations.

To determine how V and C rise from zero everywhere to the final state due to a steadily-acting arbitrary distribution of e put on at the time $t = 0$. Start with e_2 at $z = z_2$ and

none elsewhere, and let $(X + q_0 Y)A_0$ and $(X + q_1 Y)A_1$ be the currents on the left (nearest $z=0$) and right sides of the seat of impressed force. We have to find q_0, q_1, A_0, and A_1. The condition $V = Z_0 C$ at $z = 0$ gives us, by (153), (154),

$$-(X_0' + q_0 Y_0') \div S_0'' = Z_0(X_0 + q_0 Y_0);$$

therefore

$$q_0 = -(X_0' + S_0'' Z_0 X_0) \div (Y_0' + S_0'' Z_0 Y_0). \quad . \quad . \quad (159)$$

Similarly, $V = Z_1 C$ at $z = l$, gives us

$$q_1 = -(X_1' + S_1'' Z_1 X_1) \div (Y_1' + S_1'' Z_1 Y_1). \quad . \quad . \quad (160)$$

Here the numbers $_0$ and $_1$ mean that the values of X, &c. and S'' at $z = 0$ and at $z = l$ are to be taken.

Now, at the place $z = z_2$ the current is continuous, whilst the V rises by the amount e_2 suddenly in passing through it. These two conditions give us

$$(X_2 + q_0 Y_2)A_0 = (X_2 + q_1 Y_2)A_1,$$
$$-S_2'' e_2 + (X_2' + q_0 Y_2')A_0 = (X_2' + q_1 Y_2')A_1,$$

where the $_2$ means that the values at $z = z_2$ are to be taken. These determine A_0 and A_1 to be

$$A_0 \text{ or } A_1 = \frac{(X_2 + q_1 Y_2)e_2 \text{ or } (X_2 + q_0 Y_2)e_2}{(S_2'')^{-1}(X_2 Y_2' - Y_2 X_2')(q_0 - q_1)}. \quad . \quad . \quad (161)$$

Now use (155), making the denominator in (161) to be $h(q_0 - q_1)$. We have then, if C_0 and C_1 are the currents on the left and right sides of the seat of impressed force,

$$\left. \begin{aligned} C_0 &= \frac{(X + q_0 Y)(X_2 + q_1 Y_2)}{h(q_0 - q_1)} e_2, \\ C_1 &= \frac{(X + q_1 Y)(X_2 + q_0 Y_2)}{h(q_0 - q_1)} e_2. \end{aligned} \right\} \quad . \quad . \quad (162)$$

These are, when the p is throughout treated as d/dt, the ordinary differential equations of C_0 and C_1 arising out of the partial differential equation of C by subjecting it to the terminal conditions and to the impressed force discontinuity. Now make use of the algebraical expansion

$$\frac{f(p_0)}{\phi(p_0)} = \Sigma \frac{f(p)}{(p_0 - p)\dfrac{d\phi}{dp}}, \quad . \quad . \quad . \quad (163)$$

the summation being with respect to the p's which are the roots of $\phi(p)=0$, without inquiring too curiously into its strict applicability, or bothering about equal roots. Here p_0 has to be d/dt and the p's the roots of

$$\phi=h(q_0-q_1)=0 \; ;$$

so that (162) expands to

$$C=\Sigma\frac{(\mathrm{X}+q\mathrm{Y})(\mathrm{X}_2+q\mathrm{Y}_2)}{h\dfrac{d}{dp}(q_0-q_1)}\frac{e_2}{\dfrac{d}{dt}-p}, \quad . \quad . \quad (164)$$

where the single q takes the place of the previous q_0 or q_1, which have now equal values, and C has the same expression on both sides of the seat of impressed force. But e_2 is constant with respect to t, whilst C is initially zero; hence

$$\frac{e_2}{d/dt-p}=\frac{e_2(1-\epsilon^{pt})}{-p},$$

which brings (164) to

$$C=\Sigma\frac{(\mathrm{X}+q\mathrm{Y})(\mathrm{X}_2+q\mathrm{Y}_2)}{-p\dfrac{d\phi}{dp}}e_2(1-\epsilon^{pt}) \; . \quad . \quad (165)$$

which is the complete solution. By integration with respect to z we find the effect due to a steady arbitrary distribution of e put on at $t=0$; thus

$$C=\Sigma\frac{w\displaystyle\int_0^l ewdz}{-p\phi'}(1-\epsilon^{pt}), \quad . \quad . \quad . \quad (166)$$

where $\phi'=d\phi/dp$, and w is the normal current-function $\mathrm{X}+q\mathrm{Y}$. To express the V solution, turn the first w into u. The extension to e variable with t, as in Part III., is obvious. But as the only practical case of e variable with t is the case of periodic e, whose solution can be got immediately from the equations (162) by putting $p^2=-n^2$, constant, the extension is useless. Note that q_0 and q_1 are not equal in (162), and therefore in the periodic solution obtained from (162) direct they must be both used.

The quantity $-\phi'$ which occurs here is identical with the former complete $2(\mathrm{U}-\mathrm{T})$ of the line and terminal apparatus of (157) or (158).

Let C_0 be the finally reached steady current. By (166) it is

$$C_0 = \Sigma \left(-\frac{w}{p\phi'} \right) \int_0^l ewdz. \quad . \quad . \quad . \quad (167)$$

To this apply (163), with $p_0 = 0$. Then a finite expression for C_0 is

$$C_0 = (w_0/\phi_0) \int_0^l ew_0 dz, \quad . \quad . \quad . \quad (168)$$

where w_0 and ϕ_0 are what w and ϕ become when $p = 0$ in them. Or, rather, it would be so if q_0 and q_1 taken as identical could be consistent with $p = 0$. But this is not generally true, so that (168) is wrong. To suit our present purpose, we must write, by (162),

$$C_0 = \Sigma \frac{1}{-p\phi'} \left\{ (X + q_1 Y) \int_0^z e(X + q_0 Y) dz + (X + q_0 Y) \int_z^l e(X + q_1 Y) dz \right\}$$

$$= \Sigma(-p\phi')^{-1} \left\{ w_1 \int_0^z ew_0 dz + w_0 \int_z^l ew_1 dz \right\} ; \quad . \quad . \quad (169)$$

the q_0 being used in w_0, the q_1 in w_1. Now we can take $p = 0$, and get the correct formula to replace (168), viz.

$$C_0 = \frac{1}{\phi_0} \left\{ w_{10} \int_0^z ew_{00} dz + w_{00} \int_z^l ew_{10} dz \right\} ; \quad . \quad (170)$$

the second $_0$ meaning that $p = 0$ in w_0 and w_1.

If there is no leakage ($K = 0$ in S''), C_0 becomes a constant, given by

$$C_0 = \int_0^l edz \div \left\{ \int_0^l R dz + R_0 + R_1 \right\}, \quad . \quad . \quad (171)$$

where the numerator is the total impressed force, and the denominator the total steady-flow resistance ; R, R_0, and R_1 being what R'', $-Z_0$, and Z_1 become when $p = 0$ in them.

But when there is leakage (170) must be used ; it would require a very special distribution of impressed force to make C_0 the same everywhere. To find the corresponding distribution of V, say V_0, in the steady state, we have then

$$-dC_0/dz = KV_0,$$

so that a single differentiation applied to (170) finds V_0.

Knowing thus C_0 finitely, we may write (166) thus,

$$C = C_0 - \Sigma(-w/p\phi') \int_0^l ew\, dz \cdot \epsilon^{pt}, \quad . \quad . \quad (172)$$

where C_0 is given in (170). The summation here, with $t = 0$, is therefore the expansion of C_0.

The internal state of the wire is to be got by multiplying the first w by such a function of r, distance from the axis, and of whatever other variables may be necessary, as satisfies the conditions relating to inward propagation of magnetic force, and whose value at the boundary is unity. In the simple case a round solid wire, (172) becomes, by (87), Part II.,

$$C_r = C_{0r} - \Sigma \frac{r}{a_1} \frac{J_1(s_1 r)}{J_1(s_1 a_1)} \frac{w \int ew\, dz}{(-p\phi')} \epsilon^{pt}. \quad . \quad . \quad (173)$$

This gives C_r the current through the circle of radius r, less than a_1 the radius of the wire, C_{0r} being the final value. The value of s_1 is $(-4\pi\mu_1 k_1 p)^{\frac{1}{2}}$. Here of course we give to μ_1, k_1, and a_1 their proper values for the particular value of z. As before remarked, they must only vary slowly along z.

In the case of a wire of elliptical section it is naturally suggested that the closed curves taking the place of the concentric circles defined by $r = $ constant in (173) are also ellipses; and that in a wire of square section they vary between the square at the boundary and the circle at the axis. The propagation of current into a wire of rectangular section, to be considered later, may easily be investigated by means of Fourier series, at least when the return current closely envelops it.

As an explicit example of the previous, let us, to avoid introducing new functions, choose the electrical data so that the current-functions X and Y are the J_0 and K_0 functions. This can be done by letting R'' be proportional and S'' inversely proportional to the distance from one end of the line. Let there be no leakage, and

$$R'' = R_0'' z, \quad S = S_0 z^{-1};$$

where S_0 is a constant, and R_0'' a function of d/dt, but not of z. The electromagnetic and electrostatic time-constants do not vary from one part of the line to another. The equation of the current-function is

$$\frac{1}{z} \frac{d}{dz}\left(z \frac{dw}{dz} \right) = R_0'' S_0 pw; \quad . \quad . \quad . \quad (152a)$$

from which we see that

$$X = J_0(fz), \quad Y = K_0(fz),$$

where

$$f = (-R_0'' S_0 p)^{\frac{1}{2}}.$$

But, owing to the infinite conductivity at the $z = 0$ end of the line, making $K_0(fz) = \infty$ there, we shall only be concerned with the J_0 function, that is, on the left side of the impressed force, in the first place. Since V is made permanently zero at $z = 0$, the terminal condition there is nugatory. So

$$w = J_0(fz), \qquad \text{and} \quad w = J_0(fz) + q_1 K_0(fz) \; ;$$
$$u = (f/S p) J_1(fz), \quad \text{and} \quad u = (f/S p)\{J_1(fz) + q_1 K_1(fz)\} \; ;$$

on the left and right sides of an impressed force, say at $z = z_2$. The value of q_1, got from the $V = Z_1 C$ condition at $z = l$, is

$$q_1 = \frac{(fl/S_0 p) J_1(fl) - Z_1 J_0(fl)}{Z_1 K_0(fl) - (fl/S_0 p) K_1(fl)}. \quad \cdot \quad \cdot \quad \cdot \quad (160a)$$

We have also

$$\frac{XY' - YX'}{S'' p} = \frac{f}{S'' p}(J_1 K_0 - J_0 K_1)(fz) = \frac{1}{S_0 p} \; ; \quad (155a)$$

and the C solution (166) becomes

$$C = \Sigma(-p\phi')^{-1} J_0(fz) \int_0^l e J_0(fz) dz \cdot (1 - \epsilon^{pt}), \quad (166a)$$

where $\phi = -q_1/S_0 p$, and q_1 is given by (160a).

If we short-circuit at $z = l$, making $Z_1 = 0$, we introduce peculiarities connected with the presence of the series of p's belonging to $f = 0$. The expression of q_1 is then, by (160a), $q_1 = -J_1(fl)/K_1(fl)$. It seems rather singular that we should have anything to do with the K_1 function, seeing that C and V are expanded in series of the J_0 and J_1 functions. But on performing the differentiation of ϕ with respect to p it turns out to be all right, the denominator in (166a) becoming

$$-p\phi' = -\tfrac{1}{2}l^2 J_0^2(fl) \frac{d}{dp}(R_0'' p)$$

in general ; whilst in the $f = 0$ case, which makes $\phi = \tfrac{1}{2} R_0'' l^2$, we have

$$-p\phi' = -\tfrac{1}{2} p l^2 \frac{dR_0''}{dp}.$$

The value of ϕ when $p = 0$ in it is, by inspection of the expansions of J_1 and K_1, simply $\tfrac{1}{2} R_0 l^2$, the steady-flow resist-

ance of the line; R_0 being the constant that R_0'' becomes with $p=0$. We may therefore write (166a) thus :—

$$C = \int_0^l edz \div \tfrac{1}{2}R_0 l^2 - \Sigma \int_0^l edz \cdot \epsilon^{pt} \div \left(-\tfrac{1}{2}pl^2 \frac{dR_0''}{dp}\right)$$

$$-\Sigma_f \frac{J_0(fz)\displaystyle\int_0^l J_0(fz)edz}{\tfrac{1}{2}l^2 J_0^2(fl)} \Sigma_p \frac{\epsilon^{pt}}{-\dfrac{d}{dp}(pR_0'')}, \quad . \quad (172a)$$

where the first term is C_0, the finally reached current; the following summation, extending over the p's belonging to $f=0$, is its expansion, and therefore cancels the first term at the first moment; and the third part is a double summation, extending over all the f's except $f=0$, each f term having its following infinite series of p terms. This quantity in the second line is zero initially as well as finally. If there were no elastic displacement permitted ($S_0=0$), the solution would be represented by the first line of (172a), for we should then have C independent of z, and

$$\int_0^l edz = \int_0^l R''dz \cdot C = \tfrac{1}{2}R_0'' l^2 \cdot C$$

for the differential equation of C, whose solution is plainly given by the first line. The part in the second line of (172a) is therefore entirely due to the combined action of the electro-static and electromagnetic induction.

When the impressed force is entirely at $z=l$, and of such strength as to produce the steady current C_0, and if we take $R_0'' = R + Lp$, where R and L are constants, there will be only two p's to each f, given by $f^2 = -S_0 p(R+Lp)$. The subsidence from the steady state, on removal of the impressed force, is represented by

$$C = C_0 \epsilon^{-Rt/L} - \Sigma \frac{RC_0}{R+2Lp} \frac{J_0(fz)}{J_0(fl)} \epsilon^{pt},$$

$$V = -\Sigma \frac{RC_0}{R+2Lp} \frac{J_1(fz)}{J_0(fl)} \frac{fz}{S_0 p} \epsilon^{pt};$$

where the summations range over the p's, not counting the $p=-R/L$ whose C term is exhibited separately; there is no corresponding V term. A comparatively simple solution of this nature may be of course independently obtained in a more elementary manner. On the other hand, great power is gained by the use of more advanced symbolical methods,

which, besides, seem to give us some view of the inner meaning of the expansions and of the operations producing them that is wanting in the treatment of a special problem on its own merits by the easiest way that presents itself.

Leaving, now, the question of variable electrical constants, let the line be homogeneous from beginning to end, so that R'' and S'' are functions of p, but not of z. The normal current-functions are then simply

$$X = \cos mz, \quad Y = \sin mz,$$

where m is the function of p given by $-m^2 = R''S''$, so that

$$\left.\begin{aligned}
w &= \cos mz + q \sin mz, \\
u &= (m/S'')(\sin mz - q \cos mz).
\end{aligned}\right\} \quad . \quad . \quad (174)$$

Let there be a single impressed force e_2 at $z = z_2$; then the differential equations of the currents on the left and right sides of the same, corresponding to (162), will be

$$\left.\begin{aligned}
C_0 &= (\cos mz + q_0 \sin mz) \frac{\cos mz_2 + q_1 \sin mz_2}{(m/S'')(q_0 - q_1)} e_2, \\
C_1 &= (\cos mz + q_1 \sin mz) \frac{\cos mz_2 + q_0 \sin mz_2}{(m/S'')(q_0 - q_1)} e_2,
\end{aligned}\right\} \quad (162b)$$

where q_0 and q_1 are given by

$$q_0 = -\frac{S''}{m} Z_0, \quad q_1 = \frac{(m/S'') \sin ml - Z_1 \cos ml}{(m/S'') \cos ml + Z_1 \sin ml}. \quad . \quad (160b)$$

As before, in the case of an arbitrary distribution of e we are led to the solution (165), wherein for w (and for u in the corresponding V formula) use the expressions (174), in which q is to be the common value of the q_0 and q_1 of (160b), and

$$\phi = (m/S'')(q_0 - q_1) = 0 \quad . \quad . \quad . \quad . \quad (175)$$

is the determinantal equation of the p's.

Use (170) to find the final steady current distribution. Thus, now,

$$C_0 = \left\{ (\cos mz + q_1 \sin mz) \int_0^z (\cos mz + q_0 \sin mz) e\, dz \right.$$

$$\left. + (\cos mz + q_0 \sin mz) \int_z^l (\cos mz + q_1 \sin mz) e\, dz \right\} \div \frac{m}{S''}(q_0 - q_1), \quad (176)$$

in which m, q_0, q_1, and S'' have the $p = 0$ values. They are, if $i = (-1)^{\frac{1}{2}}$,

$$S'' = K, \quad m = (-RK)^{\frac{1}{2}} = gi \text{ say},$$

if R is the steady-flow resistance of line (both conductors), and K is the conductance of the insulator, both per unit length of line ;

$$q_0 = (K/m) R_0 = -KR_0 i/g,$$

if R_0 = effective steady-flow resistance at the $z = 0$ terminals, and

$$q_1 = \frac{gi \sin gli - KR_1 \cos gli}{gi \cos gli + KR_1 \sin gli},$$

if R_1 = effective steady-flow resistance at the $z = l$ terminals.

The expression on the right side of (176) is, of course, real in the exponential form, and the steady distribution of V is got by

$$KV_0 = -dC_0/dz.$$

Using the thus obtained expressions, we reach the (172) form of C solution, and the corresponding

$$V = V_0 - \Sigma(-p\phi')^{-1} u \int_0^l ewdz . \epsilon^{pt} \quad . \quad . \quad (176\text{A})$$

The value of ϕ' here, got by differentiation with respect to p, may be written in many ways, of which one of the most useful, for expansions in Fourier series, is the following. Let

$$w = (1 + q^2)^{\frac{1}{2}} \cos(mz + \theta);$$

then

$$\frac{d\phi}{dp} = \frac{m}{S'' \cos^2\theta} \frac{d}{dp} \left\{ \tan^{-1}\left(\frac{m}{S''} \frac{Z_1 - Z_0}{(m/S'')^2 + Z_1 Z_0}\right) - ml \right\}$$

$$= \frac{l}{2S'' \cos^2\theta} \frac{dm^2}{dp} \left\{ \cos^2 ml \frac{d}{d(ml)} \left(\frac{m}{S''} \frac{Z_1 - Z_0}{(m/S'')^2 + Z_1 Z_0}\right) - 1 \right\}. \quad (177)$$

Corresponding to this,

$$\tan ml = \frac{m}{S''} \frac{Z_1 - Z_0}{(m/S'')^2 + Z_1 Z_0} \quad . \quad . \quad . \quad (178)$$

finds the angles ml; it is got by the union of

$$\tan\theta = S'' Z_0/m, \quad \tan(ml + \theta) = S'' Z_1/m, \quad . \quad . \quad (179)$$

which are equivalent to (160 *b*).

For example, if we take $R'' = R$, constant, thus abolishing inertia, and $S'' = Sp$, no leakage, and S constant (R and S not containing p, that is to say), the expansion of V_0 an arbitrary function of z is

$$V_0 = \Sigma \sin(mz + \theta) \frac{\int_0^l V_0 \sin(mz + \theta) dz}{\frac{l}{2} \left\{ 1 - \cos^2 ml \frac{d}{d(ml)} \frac{m}{Sp} \frac{Z_1 - Z_0}{(m/Sp)^2 + Z_1 Z_0} \right\}}, \quad (180)$$

subject to (178). Here $p = -m^2/\mathrm{RS}$, so that the state of the line at time t after it was V_0, when left to itself, is got by multiplying each term in the expansion by $\epsilon^{-m^2t/\mathrm{RS}}$. The corresponding current is given by $\mathrm{RC} = -d\mathrm{V}/dz$. But the solution thus got will usually only be correct, although (180) is correct, when there is, initially, no energy in the terminal apparatus. If there be, additional terms in the numerator of (180) are required, to be found by the energy-difference method of Part III. They will not alter the value of the right member of (180) at all ; they only come into effect after the subsidence has commenced. Similar remarks apply whatever be the nature of the line. It is, however, easy to arrange matters so that the energy in the terminal apparatus shall produce no effect in the line. For example, join the two conductors at one end of the line through two equal coils in parallel ; if the currents in these coils be equal and similarly directed in the circuit they form by themselves, they will not, in subsiding, affect the line at all.

Returning to (177), or other equivalent expression, it is to be observed that particular attention must be paid to the roots $ml = 0$, which may occur, or to the series of roots p belonging to the $m = 0$ case, when we are working down from the general to the special, and happen to bring in $m = 0$. Take $\mathrm{Z}_1 = 0$ for instance, making, by (175) and (160 b),

$$\phi = -\mathrm{Z}_0 - \frac{m}{\mathrm{S}p}\tan ml,$$

where $m^2 = -\mathrm{S}p\mathrm{R}''$. Then

$$\frac{d\phi}{dp} = -\frac{d\mathrm{Z}_0}{dp} - \frac{\tan ml}{2m}\left(\frac{d\mathrm{R}''}{dp} - \frac{\mathrm{R}''}{p}\right) + \frac{l}{2}\sec^2 ml\left(\frac{d\mathrm{R}''}{dp} + \frac{\mathrm{R}''}{p}\right). \quad (181)$$

Now, as long as Z_0 is finite, m cannot vanish ; but when Z_0 is zero, giving $ml =$ any integral multiple of π, $m = 0$ is one case. Then we have, when m is finite,

$$\frac{d\phi}{dp} = \frac{l}{2}\left(\frac{d\mathrm{R}''}{dp} + \frac{\mathrm{R}''}{p}\right), \quad \text{and} \quad p\frac{d\phi}{dp} = \frac{l}{2}\frac{d}{dp}(p\mathrm{R}''); \quad . \quad . \quad (182)$$

but when m is zero the middle term on the right of the preceding equation becomes finite, making

$$d\phi/dp = l(d\mathrm{R}''/dp).$$

The result is that the current solution contains a term, or infinite series, apparently following a different law to the rest, with no corresponding terms in the V solution. This merely means that the mean current subsides without causing any

electric displacement across the dielectric, when the ends are short-circuited (Z=0); so that if, in the first place, the current had been steady, and there had been no displacement, there would have been none during the subsidence.

The transition from the combined inertia and elasticity solutions to elasticity alone is very curious. Thus, let Z=0 at both ends, and $R''=R+Lp$, where R and L are constants not containing p. The rise of current due to e is shown by

$$C = \frac{\int_0^l e\, dz}{Rl}(1-\epsilon^{-Rt/L}) + \frac{2}{l} \Sigma \frac{\cos mz \int_0^l e \cos mz\, dz}{R+2Lp}\epsilon^{pt}, \quad (183)$$

the m's in the summation being π/l, $2\pi/l$, &c.; and each having two p's, given by

$$0 = m^2 + RSp + LSp^2.$$

The $m=0$ part is exhibited separately, and is what the solution would be if e were a constant (owing to the constancy of R). But, whatever e be, as a function of z, the summation comes to nothing initially, on account of the doubleness of the p's, just as in (172 a) the part in the second line vanishes by reason of every p summation vanishing when $t=0$.

Now, in (183), let L be exceedingly small. The two p's approximate to $-m^2/RS$, the electrostatic one, and to $-R/L$, the electromagnetic one, which goes up to ∞, the storehouse for roots. The current then rises thus :

$$C = \frac{\int_0^l e\, dz}{Rl}(1-\epsilon^{-Rt/L}) + \frac{2}{Rl}\Sigma \cos mz \int_0^l e \cos mz\, dz \cdot (1-\epsilon^{-Rt/L})$$
$$- \frac{2}{Rl}\Sigma \cos mz \int_0^l e \cos mz\, dz \cdot (1-\epsilon^{-m^2t/RS}). \quad (184$$

But the first line on the right side is equivalent to

$$(e/R)(1-\epsilon^{-Rt/L}),$$

and here the exponential term vanishes instantly, on L being made exactly zero, so that (184) becomes

$$C = \frac{e}{R} - \frac{2}{Rl}\Sigma \cos mz \int_0^l e \cos mz\, dz \cdot (1-\epsilon^{-m^2t/RS}), \quad (185)$$

except at the very first moment, when it gives $C=e/R$, which is quite wrong, although the preceding formula, giving $C=0$ at the first moment, is correct. Or, (185) is equivalent to

$$C = \frac{1}{R}\left(e - \frac{dV}{dz}\right),$$

from which inertia has disappeared. Here V is given by (188) below. The process amounts to taking one half the terms of the summation in (183), and joining them on to the preceding term to make up e/R, which is quite arbitrary. An alternative form of (185) is

$$\mathrm{C} = \frac{\int_0^l edz}{\mathrm{R}l} + \frac{2}{\mathrm{R}l} \Sigma \cos mz \int_0^l e \cos mz\, dz \cdot \epsilon^{-m^2 t/\mathrm{RS}}. \quad . \quad (186)$$

On the other hand, there is no such peculiarity connected with the V solution in the act of abolishing inertia. The $m=0$ term is

$$-\frac{1}{\mathrm{R}l}\left(\frac{m}{\mathrm{S}p} \sin mz \int_0^l edz\right) = 0,$$

because m is zero and p finite. Therefore V rises thus,

$$\mathrm{V} = \frac{2}{l} \Sigma \frac{m \sin mz \displaystyle\int_0^l e \cos mz\, dz}{-\mathrm{S}p(\mathrm{R}+2\mathrm{L}p)} (1-\epsilon^{pt}), \quad . \quad (187)$$

before abolition of inertia. But as L is made zero, the denominator becomes m^2 for the electrostatic p, and ∞ for the other; thus one half the terms vanish, leaving

$$\mathrm{V} = \frac{2}{l} \Sigma \frac{\sin mz}{m} \int_0^l e \cos mz\, dz(1-\epsilon^{-m^2 t/\mathrm{RS}}), \quad . \quad (188)$$

where $\mathrm{L}=0$, without any of the curious manipulation to which the current formula was subjected.

Next let us consider the transition from the combined elasticity and inertia solution to inertia alone (of course with resistance in both cases, as in the preceding transition). It is usual to wholly ignore electrostatic induction in investigations relating to linear circuits. This is equivalent to taking $\mathrm{S}=0$, stopping elastic displacement, and compelling the current to keep *in* the wires always, *i. e.* when the insulation is perfect, as will be here assumed. We then have, by (145),

$$-\frac{d\mathrm{C}}{dz} = 0, \qquad e - \frac{d\mathrm{V}}{dz} = \mathrm{R}''\mathrm{C}. \quad . \quad . \quad . \quad (189)$$

By integrating the second of these with respect to z we get rid of V, and obtain the differential equation of C,

$$\int_0^l edz = \left\{ \int_0^l \mathrm{R}''dz + \mathrm{Z}_1 - \mathrm{Z}_0 \right\} \mathrm{C} = \phi_1 \mathrm{C}, \text{ say}, \quad . \quad (190)$$

whence follows this manner of rise of the current, when e is

steady, and put on everywhere at the time $t=0$, reaching the final value C_0,

$$C = C_0 - \Sigma \left(-p\frac{d\phi_1}{dp}\right)^{-1} \int_0^l edz \cdot \epsilon^{pt}, \quad . \quad . \quad . \quad (191)$$

$\phi_1 = 0$ finding the p's. We can find V at distance z by integrating the second of (189) with respect to z from 0 to z; thus

$$V = \int_0^z edz + (Z_0 - \int_0^z R''dz)C, \quad . \quad . \quad . \quad (192)$$

wherein C is to be the right member of (191). This finds V by differentiations with respect to t performed on C. In the final state put R_0'' for R'' and $-R_0$ for Z_0, steady-flow resistances. V will usually vary with the time until the steady state is reached; but if the line is homogeneous, with only the two constants R and L, and if also Z_0 and Z_1 are zero, V will be independent of t, and instantly assume its final distribution.

Thus, on these assumptions, we shall have

$$\left. \begin{array}{l} C = (\int_0^l edz/Rl)(1-\epsilon^{-Rt/L}), \\[2mm] V = \int_0^z edz - (z/l)\int_0^l edz, \end{array} \right\} \quad . \quad . \quad . \quad (193)$$

showing the current to rise independently of the distribution of e, and V to have its final distribution from the first moment, which, when the impressed force is wholly at $z=0$, of amount e_0, is $e_0(1-z/l)$. This infinitely rapid propagation of V is common sense according to the prescribed conditions, but absolute nonsense physically considered, especially in view of the transfer of energy. The question then arises, How does V really set itself up, when the line is so short that the current rises sensibly according to the electromagnetic theory?

To examine this, let the line constants be R, S, L (independent of d/dt), and $Z_1 = Z_0 = 0$. Put on e_0 at $z=0$ at time $t=0$. V and C will rise thus (a special case of (183) and (187)),

$$\left. \begin{array}{l} C = \dfrac{e_0}{Rl}(1-\epsilon^{-Rt/L}) + \dfrac{2e_0}{Rl}\epsilon^{-Rt/2L}\,\Sigma\,\dfrac{2}{m'}\cos mz \sin\dfrac{Rm't}{2L}, \\[3mm] V = e_0\left(1-\dfrac{z}{l}\right) - \dfrac{2e_0}{l}\epsilon^{-Rt/2L}\,\Sigma\,\dfrac{\sin mz}{m}\left(\cos+\dfrac{\sin}{m'}\right)\dfrac{Rm't}{2L}, \end{array} \right\} \quad (194)$$

where m has the values π/l, $2\pi/l$, &c., and

$$m' = (4m^2L/R^2S-1)^{\frac{1}{2}}.$$

It is clear that when S is made to vanish, making $m' = \infty$, the current oscillations wholly vanish, reducing the C solution to the first of (193). But the V oscillations remain in full force, though of infinitely short period, and subside at a definite rate. This means that the mean value of V at any place has to be taken to represent its actual value, and this mean value is its final value. That is, if $\overline{V}$ denote the mean value, about which V oscillates, we have

$$\overline{V} = e_0(1 - z/l) = V_0.$$

Introduce $LS = v^{-2}$, where v is constant, making

$$m' = 2mLv/R$$

very nearly, when the line is short; then the second of (194) becomes

$$V = e_0\left(1 - \frac{z}{l}\right) - \frac{2e_0}{l}\,\epsilon^{-Rt/2L}\,\Sigma\,\frac{\sin mz}{m}\cos mvt, \quad . \quad (195)$$

which must very nearly show the subsidence of the oscillations. First ignore the subsidence factor, replacing it by unity, then (195) represents a wave of V travelling to and fro at velocity v, as thus expressed,

$$\left.\begin{array}{l} V = e_0 \text{ from } z = 0 \text{ to } z = vt, \\ V = 0 \text{ beyond } z = vt. \end{array}\right\} \text{ when } vt < l.$$

When $vt = l$, the whole line is charged to $V = e_0$. The wave then moves back in the same manner as it advanced, so that the state of things at time $t = l/v + \tau$ is the same, until t reaches $2l/v$, when we have $V = 0$ as at first. This would be repeated over and over again if there were no resistance, which, through the exponential factor, causes the range of the oscillations of V at any place about the final value to diminish according to the time constant $2L/R$. Also, the resistance has the effect of rounding off the abrupt discontinuity in the wave of V.

I have given a fuller description of this case elsewhere (Journal S. T. E. and E. vol. ix., " On Induction between Parallel Wires "), and only bring it in here in connection with the interpretation according to my present views regarding the transfer of energy. As it is clear that this oscillatory phenomenon is, primarily, a dielectric phenomenon, and only affects the conductor secondarily, it is necessary that the L in the above should not at the beginning be the full L of dielectric and wires, but only L_0, that of the dielectric, making v the velocity of undissipated waves, although as the oscillations subside the velocity must diminish, tending towards $v = (LS)^{-2}$, which may, however, be far from being reached, especially in

the case of an iron wire. The nature of the dielectric wave
is far more simply studied graphically than by means of
Fourier series, on the assumption of infinite conductivity,
which allows us to represent things by means of two oppo-
sitely travelling waves. To this I may return in the next Part.

I will conclude the present Part with a brief outline of the
reasoning which guided me six months ago, when my brother's
experiments on induction between distant circuits (mentioned
in Part II.) in the north of England commenced, to the con-
clusion that long-distance signalling (*i. e.* hundreds of miles)
was possible by induction, a conclusion which has been some-
what supported by results, so far as the experiments have yet
gone. Recognizing the great complexity of the problem, and
the difficulty of hitting the exact conditions, I made no special
calculations but preferred to be guided by general considera-
tions ; for, in the endeavour to be precise when the data are
uncertain and very variable, one is in great danger of swallow-
ing the camel.

One may be fairly well acquainted with electromagnetism,
and also with the capabilities of the telephone, and yet receive
the idea of signalling by induction long distances with utter
incredulity, or at least in the same way as one might accept
the truth of the statement, that when one stamps one's foot
the universe is shaken to its foundations. Quite true, but
insensible a few yards away. The incredulity will probably
be based upon the notion of rapid decrease with distance of
inductive effects. This, however, leaves out of consideration
an important element, namely the size of the circuits.

The coefficients of electromagnetic induction of linear cir-
cuits are proportional to their linear dimensions. If, then, we
increase the size of two circuits n times, and also their distance
apart n times, the mutual inductance M is increased n times.
Let R_1 and R_2 be the resistances of primary and secondary.
The induced current (integral) in the secondary due to start-
ing or stopping a current C_1 in the primary is MC_1/R_2, or
Me_1/R_1R_2, if e_1 be the impressed force in the primary. Now
increasing the linear dimensions, and the distance, in the
ratio n (with the same kind of wire) increases M, R_1, and R_2
all n times. So only e_1 remains to be increased n times to get
the same secondary-current impulse. We can therefore en-
sure success in long-distance experiments on the basis of the
success of short-distance experiments, with elements of uncer-
tainty arising from new conditions coming into operation at
the long distances.

But practically the result must be far more favourable to

the long than to the short distances than the above asserts. For no one, when multiplying the distance and size of circuits, say ten times, would think of putting ten telephones in circuit to keep rigidly to the rule. Thus it may be that only a slight increase of e_1 is required, on account of M being multiplied in a far greater ratio than the resistances, or the self-inductances. Thus, it is not uncommon for the R and L of a telephone to be 100 ohms and 12 million centim. These form the principal parts of the R and L of a circuit of moderate size, and of course do not increase when we enlarge the circuit. It is therefore certain that we can signal long distances on the above basis, with a margin in favour of the long distances, which will be large or small according as the circuits are small or large.

Again, if e_1 in the primary be periodic, of frequency $n/2\pi$, the ratio of the amplitude of the current in the secondary to that in the primary will be

$$\mathrm{M}n \div (\mathrm{R}_2^2 + \mathrm{L}_2^2 n^2)^{\frac{1}{2}}.$$

Now, without any statement of the magnitude of the current in the primary, if it be largely in excess of requirements for signalling in the primary, so that $\frac{1}{100}$ part, say, would be sufficient for the purpose, then we shall have enough current in the secondary if the above ratio is only $\frac{1}{100}$. But, without going to precise formulæ, it may be easily seen that the above ratio may be made quite a considerable fraction, in comparison with $\frac{1}{100}$, with closed metallic circuits whose linear dimensions and distance are increased in the same ratio. But we should expect a rapid decrease of effect when the mean distance between the circuits exceeds their diameter, keeping the circuits unchanged. (It should be understood that squares, circles, &c. are referred to.)

The theory seems so very clear (though it is only the first approximation to the theory), that it would be matter for wonder and special inquiry if we found that we could not signal long distances by induction between closed metallic circuits, starting on the basis of a short-distance experiment, and following up the theory.

[As a matter of fact, it was found possible to speak by telephone between two circuits of $\frac{1}{4}$ mile square, $\frac{1}{2}$ mile between centres, using two bichros with the microphone.]

Now coming to metallic lines whose circuits are closed through the earth, the theory is rendered far more difficult on account of there being a conduction-current from the primary to the secondary due to the earth's imperfect conductivity. We therefore have, to say nothing of electrostatic induction,

a superposition of effects due to induction and conduction, the latter being far more difficult to theoretically estimate than the former.   But the reasoning regarding the electro-magnetic induction is not very greatly changed, although not so favourable to long-distance signalling.   If the return currents diffused themselves uniformly in all directions from the ends of the line, the same property of *n*-fold increase of M with *n*-fold lengthening of the lines and their distance would still be true.   But the diffusion is one-sided only, and is even then only partial, especially when exceedingly rapid alternations of current take place.   But we have the power of counterbalancing this by the multiplication of the variations of current in the primary that we can get by making and breaking the circuit, with a considerable battery-power if necessary, getting something enormous compared with the feeble variations of current in the microphonic circuit, or that can work a telephone.   Electrostatic induction also comes in to assist, as it increases the activity of the battery, and therefore the current in the secondary also.

But, as regards wires connected to earth, this does not profess to be more than the very roughest reasoning, though in my opinion quite plain enough to show that we may ascribe the signalling across 40 miles of country between lines about 50 miles long mainly to induction, as we should be necessitated to do if we carried the experiment further and closed the circuits metallically by roundabout courses, for then the plain argument relating to induction will become valid.   Experiments of this kind are of the greatest value from the theoretical point of view, and it is to be hoped that they will be greatly extended.

LI. *Note on the Effect of Stress and Strain on the Electrical Resistance of Carbon.   By* Herbert Tomlinson, *B.A.*[*]

PROFESSOR T. C. MENDENHALL has published in the September number of Silliman's American Journal, and also in the October number of the Philosophical Magazine, an account of some experiments on the effect of pressure on the electrical resistance of carbon.   These experiments deal not only with such comparatively hard rods of carbon as are used in the arc lamp, but also with the compressed lamp-black seen in Edison's disks.   I will first refer to the experiments on the hard carbon.   Prof. Mendenhall seems to think that these are in accordance with some experiments made by

* Communicated by the Author.

myself*; but this I rather doubt, for the change of resistance produced by pressure on the carbon rod used by him would appear to be considerably greater than any which was obtained by myself on carbon rods of a similar kind. In fact, had I not employed a very sensitive arrangement for testing small changes of electrical resistance †, I might have failed in detecting *any* change whatever. The result arrived at by myself was that a pressure of 1 grm. per square centim. would produce a *percentage* decrease of only ·00000064. So that, if we assume the decrease of resistance to be proportional to the pressure, there would be required a pressure of 22,215 lb. on the square inch to produce a decrease of resistance of 1 per cent. I am not aware of any investigations on the pressure which would suffice to crush carbon, but we may form some rough notion of its amount in the following manner :—The crushing pressure of wrought iron is 36,000 lb., and the value of Young's modulus for the metal is about 1900×10^6 grm. per square centim., whilst the corresponding value for this particular specimen of carbon was $267 \cdot 2 \times 10^6$ grm. per square centim. We may perhaps say that the crushing pressure of the carbon would probably be somewhere about

$$\frac{36,000 \times 267 \cdot 2}{1900} \text{ lb. per square inch,}$$

i. e. about 5000 lb. per square inch. If this result be at all correct, it would follow that pressure on the point of *crushing* the carbon would diminish the electrical resistance by *less than $\frac{1}{4}$ per cent.* Prof. Mendenhall does not give any data by which the amount of change of resistance observed by him can be calculated ; but, if one may judge from the tenour of his remarks and his arrangements for experimenting, it would seem probable that the ratio of change of resistance to pressure was considerably greater in his experiments than in mine.

Further, Prof. Mendenhall seems to credit me with doing what I have not done ; for, after describing his own experiments on the effect of pressure exerted in the same direction as the current, he goes on to say, " It was also found that compression at right angles to the direction of the current produced a similar effect, but less in magnitude. These facts had been already announced by Mr. Tomlinson." I have made no experiments on the effect of *compression at right angles to the direction of the current*, but I think it probable that if I had I should have found *increase* of resistance, and

* Phil. Trans., Part I. 1883, p. 65.
† With the carbon I was able not only to detect but to measure a change of resistance of about 1 in 100,000.

not *decrease*, to follow on compression exerted in the above direction.

On the whole, then, I am of opinion that my experiments are more in accordance with those of Professors Sylvanus Thompson, W. F. Barrett, and others, than with those of Prof. Mendenhall.

As regards soft carbon, such as is used with Edison's disks, I cannot speak from experience. I was, however, much interested in reading the account given by Prof. Mendenhall of his investigations on the subject, and especially with that part of the account which relates to the influence of *time* on the change of resistance of the carbon disk when the pressure is varied ; inasmuch as some of my own experiments have shown a similar influence of time on the electrical resistance of the *viscous* metals zinc and tin in the form of foil, when traction was applied in a direction transverse to that of the current[*].

At the same time I would venture to suggest to Prof. Mendenhall, that his ingenious experiments may not be quite conclusive as to a considerable change taking place in the *specific* resistance of the *carbon.* After showing that very considerable change does take place *somewhere* in the circuit of the mercury in contact with the carbon button and the button itself, he substitutes for the soft carbon a disk of hard carbon similar in dimensions, and finds that a pressure of *only 7 centimetres of mercury* lowers the resistance by *nearly 3 per cent.*; and he says " There can be little doubt that this small reduction[†] is due almost entirely[‡] to better surface-contact produced by pressure." From the above it is evident that the surface-contact between the mercury and hard carbon was by no means good ; but, writes Prof. Mendenhall, " The faces of a soft carbon disk are always smooth and polished ; the surface of hard carbon, on the contrary, is generally more or less rough and irregular. It would appear, therefore, that, if the reduction of the resistance of soft carbon by increase of pressure is due to better surface-contact, this reduction of resistance should be much more marked with hard than with soft carbon." The above assumption is a very dangerous one

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[*] *Loc. cit.* pp. 68 and 69.

[†] Small in comparison to that which ensued with the soft carbon, which was about twenty times as great for the same pressure.—H. T.

[‡] The words " almost entirely " used here seem to indicate that in the experiments described in the first part of this note Prof. Mendenhall found much greater changes produced in his carbon than I did. Such a pressure as that of 7 centimetres of mercury would not have produced *any sensible* effect even with my arrangement, which must have been much more sensitive than that of Prof. Mendenhall.—H. T.

to make, for the surface of the soft carbon might not be in the same state of cleanliness as that of the hard carbon.

But even suppose we allow that there was better contact with the mercury in the case of the soft carbon than in that of the hard carbon, it does not necessarily follow that the observed decrease of resistance consequent on increase of pressure occurred in the *carbon* of the button. The buttons are, I believe, formed by compressing lampblack *mixed with gum-water*. Must we not suppose, then, that the particles of lampblack are bound to each other by the gum, and separated from each other by the gum, to a greater or less extent? Would not the diminution of resistance experienced in the body of the button when pressure was applied be due to one or both of the two following causes :—

1. Diminution of the resistance of the thin coating of gum between the particles of carbon ?

2. Better surface-contact between one particle of carbon and another ?

The influence of *time* on the change of resistance might be quite accounted for by supposing cause 1 to be at work*.

LII. *On Stationary Waves in Flowing Water.*—Part II. *By* Sir WILLIAM THOMSON, *F.R.S. &c.*

[Continued from p. 357.]

Correction in Part II.—In lines 4–7 of paragraph 3 on page 357, delete the words ", because" ... to ... " canal "; and add the sentence " The explanation of this will be more fully developed in Part III., to be published in December."

TO find, as promised in Part I., the sum of horizontal pressures on an inequality of the bottom, or on a bar, or on a series of inequalities or bars, consider the horizontal components of momentum of different portions of the water in the following manner. Because the motion is steady, the momentum of the matter at any instant within any fixed volume of space S remains constant; and therefore the rate of delivery of momentum from S by water flowing out on one side above gain of momentum by water flowing into S on the other side must be equal to the total amount of horizontal force acting on the water which at any instant is within S ; the direction of this force being that of the flow when the momentum of the leaving water exceeds that of the entering water. Now let S be the space bounded by the bottom, the

* See the account of my own experiments on the *viscous* metals zinc and tin, quoted above.

free surface of the water, and four vertical planes, two of them, called A_0, A, perpendicular to the stream, and two of them parallel to the stream and at unit distance from one another.   Let $\mathfrak{P}$ P B, and $\mathfrak{P}_0$ B_0 be vertical lines on the two transverse ends A and A_0 of the space S ; $\mathfrak{P}$, $\mathfrak{P}_0$ being points of the surface, and B, B_0 points of the bottom.   Let

$$\mathfrak{P}B = D \text{ and } \mathfrak{P}P = y,$$

and let u be the horizontal component velocity at P.   The rate of delivery of momentum (per unit of time understood) from S by water flowing across A is equal to

$$\int_0^D u^2 dy \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1);$$

and the excess of delivery of momentum from S across A above receipt of momentum across A_0 is equal to

$$\int_0^D u^2 \, dy - \left\{ \int_0^D u^2 \, dy \right\}_0 . \quad . \quad . \quad . \quad . \quad (2).$$

When this is positive, the water between A_0 and A must experience, on the whole, a pressure in the direction from A_0 towards A, made up of difference of fluid-pressures on the end sections A_0 and A, and pressures upon the water by fixed inequalities, if there are any, between A_0 and A. Hence if X, X_0 denote the integral fluid-pressures on the ideal planes A, A_0, and F the sum of horizontal pressures of the inequalities on the fluid, regarded as positive when the direction of the total is from A towards A_0, (2) must be equal to

$$X_0 - X - F \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Hence we have

$$F = \left\{ X + \int_0^D u^2 dy \right\}_0 - \left(X + \int_0^D u^2 dy \right) \quad . \quad . \quad (4).$$

Now the fluid-pressure at P is equal to $gy + \frac{1}{2}(\mathfrak{q}^2 - q^2)$, by the elementary formula for pressure in steady motion (the pressure at the free surface being taken as zero), $\mathfrak{q}$ and q denoting the velocity of the fluid at $\mathfrak{P}$ and P respectively. Hence

$$X = \int_0^D [gy + \tfrac{1}{2}(\mathfrak{q}^2 - q^2)] dy = \tfrac{1}{2}(gD + \mathfrak{q}^2)D - \tfrac{1}{2}\int_0^D q^2 dy \ . \ (5).$$

Hence

$$X + \int_0^D u^2 dy = \tfrac{1}{2}(gD + {}^2)\underline{D + \tfrac{1}{2}}\int_0^D (u^2 - v^2) dy \quad . \quad . \quad (6),$$

if v be the vertical component velocity at P.

This, and the corresponding expression relatively to A_0, gives, by (3), the sum of horizontal pressure on all inequalities between A_0 and A, when the problem of the fluid motion in the circumstances is so far solved as to give D, $\mathfrak{q}$, and $u^2 - v^2$ for each of the end sections A_0, A.

Suppose, now, A_0 to be so far on the up-stream side of the inequalities that the motion of the water across it is sensibly uniform and horizontal, with velocity which we shall denote by U_0; so that, for A_0, (6) becomes

$$\left\{ X + \int_0^D u^2 dy \right\}_0 = \tfrac{1}{2} g D_0^2 + U_0^2 D_0 \quad . \quad . \quad . \quad (7).$$

Hence, and by (6) and (4),

$$F = \tfrac{1}{2} g (D_0^2 - D^2) + U_0^2 D_0 - \tfrac{1}{2} \mathfrak{q}^2 D - \tfrac{1}{2} \int_0^D (u^2 - v^2) dy \quad . \quad (8).$$

Now, by the law of velocity at the free surface in steady motion, we have

$$\tfrac{1}{2} \mathfrak{q}^2 = \tfrac{1}{2} U_0^2 + g (D_0 - D) \quad . \quad . \quad . \quad (9);$$

because, the points B_0, B of the bottom being on the same level, $D_0 - D$ is the difference of levels between the surface-points $\mathfrak{P}_0$ and $\mathfrak{P}$. Hence (8) becomes

$$F = \tfrac{1}{2} g (D_0 - D)^2 + U_0^2 (D_0 - D) - \tfrac{1}{2} (U^2 - U_0^2) D$$
$$+ \tfrac{1}{2} \int_0^D (v^2 + U^2 - u^2) dy \quad . \quad (10),$$

where U denotes a constant which may have any value. It is convenient to make it the mean horizontal component velocity across $\mathfrak{P}B$: we therefore take

$$U = \frac{1}{D} \int_0^D u \, dx \quad . \quad . \quad . \quad . \quad (11):$$

and, because the quantities flowing in across A_0 and out across A are equal, as the motion is steady, we have
$$UD = U_0 D_0 \quad . \quad . \quad . \quad . \quad . \quad (12).$$
Using this to eliminate U_0 from (10), we find

$$F = \tfrac{1}{2} \left(g - \frac{U^2 D}{D_0^2} \right) (D_0 - D)^2 + \tfrac{1}{2} \int_0^D (v^2 + U^2 - u^2) dy \quad . \quad (13).$$

To evaluate $D_0 - D$ when we know enough about the motion, and to see how its value is related to other characteristic quantities, let us look back to (9), and in it take

$$\mathfrak{q}^2 = U^2 + \mathfrak{v}^2. \quad . \quad . \quad . \quad . \quad (14).$$

Thus, if $\mathfrak{P}$ be chosen at a point of the water-surface where the horizontal component velocity is rigorously or approximately equal to U, then $\mathfrak{v}$ is rigorously or approximately the vertical component velocity at $\mathfrak{P}$. Using now (14) in (9), with UD/D_0 for U_0, we find

$$D_0 - D = \frac{\frac{1}{2}\mathfrak{v}^2}{g - \frac{\frac{1}{2}(D_0 + D)U^2}{D_0^2}} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (15);$$

which, used in (13), gives

$$F = \frac{\mathfrak{v}^4}{8} \frac{g - \dfrac{U^2 D}{D_0^2}}{\left[g - \dfrac{\frac{1}{2}(D_0 + D)U^2}{D_0^2} \right]^2} + \frac{1}{2} \int_0^D (\mathfrak{v}^2 + U^2 - u^2) dy \;.\; (16).$$

Hence, when the change of level, $D_0 - D$, is but small, in comparison with D or D_0, we have

$$F \doteqdot \frac{1}{8} \frac{\mathfrak{v}^4}{g - \dfrac{U^2}{D}} + \frac{1}{2} \int_0^D (\mathfrak{v}^2 + U^2 - u^2) dy \;.\;.\; (17),$$

where $\doteqdot$ denotes approximate equality. Going back to (16), let $\mathfrak{P}$ be so chosen on the water-surface that

$$\int_0^D u^2 dy = U^2 D \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (18),$$

which it is clear we can do, because at a crest the first member is less than the second, and at a hollow greater. When the motion is infinitely nearly simple harmonic (the stream-lines curves of lines), the position of $\mathfrak{P}$ thus chosen will be exactly the middle between crest and hollow. When the motion is anything, however great, up to Stokes's highest possible wave, the chosen place of $\mathfrak{P}$ is a less or more rough approximation to the mid-level point of a wave : it is always rigorously determinate. For brevity we shall call it, that is to say a point defined by (18), a nodal point. Thus, when $\mathfrak{P}$ is taken as a nodal point, (16) becomes simplified to

$$F = \frac{\mathfrak{v}^4}{8} \frac{g - \dfrac{U^2 D}{D_0^2}}{\left[g - \dfrac{\frac{1}{2}(D_0 + D)U^2}{D_0^2} \right]^2} + \frac{1}{2} \int_0^D \mathfrak{v}^2 dy \;.\;.\; (19).$$

This expression is rigorous. In it $\mathfrak{v}$, which is given rigorously by (14), is approximately (not rigorously) equal to the vertical component velocity at $\mathfrak{P}$: and if we suppose D given,
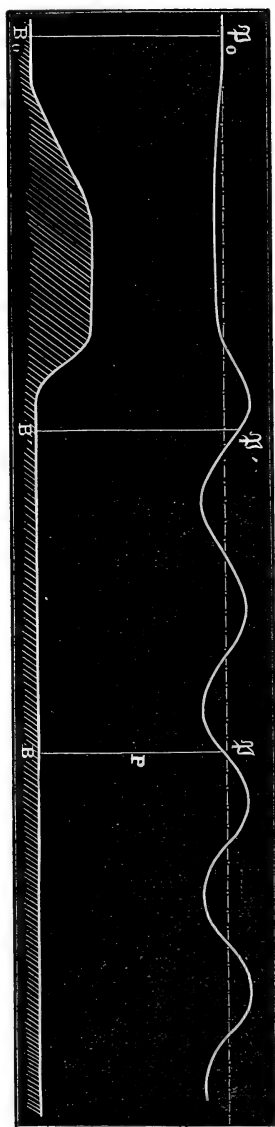
Fig. 2.

Fig. 1.
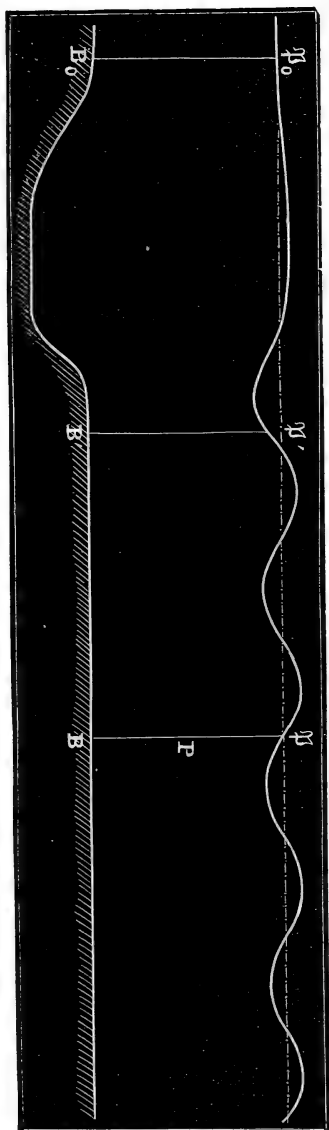
D_0 is found by (15), which is a cubic equation in D_0, most easily solved by successive approximations according to the process obviously indicated by the form in which the equation appears in (15). (As a first approximation take D for D_0 in the second member, and so on.)

To work out the formula (19) for the case of infinitesimal displacement, we may take $\mathfrak{P}$ at a great enough distance from inequalities to let the surface in its neighbourhood be sensibly a curve of sines, and the motion simple harmonic. The investigation is facilitated by also taking $\mathfrak{P}$ at a node, as in the diagrams. If we take

$$\mathfrak{y} = h \sin mx \quad . \quad . \quad . \quad . \quad . \quad (20)$$

as the equation of the free surface, the known solution for simple harmonic waves in water of depth D gives,

$$\left. \begin{array}{l} u = U \left\{ 1 + mh \, \dfrac{\epsilon^{m(D-y)} + \epsilon^{-m(D-y)}}{\epsilon^{mD} - \epsilon^{-mD}} \sin mx \right\}, \\[3mm] v = \qquad U mh \, \dfrac{\epsilon^{m(D-y)} - \epsilon^{-m(D-y)}}{\epsilon^{mD} - \epsilon^{-mD}} \cos mx, \end{array} \right\} \quad . \quad (21).$$

where $\qquad U = \sqrt{\left\{ \dfrac{g}{m} \, \dfrac{\epsilon^{mD} - \epsilon^{-mD}}{\epsilon^{mD} + \epsilon^{-mD}} \right\}}.$

Hence, where $x = 0$, as in the nodal section $\mathfrak{P}$ P B,

$$u = U, \quad \text{and} \quad v = U mh \, \frac{\epsilon^{m(D-y)} - \epsilon^{-m(D-y)}}{\epsilon^{mD} - \epsilon^{-mD}} \quad . \quad . \quad (22);$$

also

$$\int_0^D v^2 dy = \tfrac{1}{2} U^2 mh^2 \frac{\epsilon^{2mD} - \epsilon^{-2mD} - 4mD}{(\epsilon^{mD} - \epsilon^{-mD})^2} \quad . \quad . \quad . \quad (23),$$

$$= \tfrac{1}{2} gh^2 \left\{ 1 - \frac{4mD}{\epsilon^{2mD} - \epsilon^{-2mD}} \right\} \quad . \quad . \quad (24).$$

Now going back to (19) we see that when U approaches the critical velocity $\sqrt{gD \dfrac{D_0^2}{\tfrac{1}{2}(D_0 + D)D}}$, the first term might become important, even though the corrugations at a great distance down-stream from the inequalities were infinitesimal. Reserving consideration of this case, and supposing for the present U to be considerably smaller than the critical value, we may neglect the first term in comparison with the second, remembering that in fact quantities comparable with the first

term are neglected in the approximation (24) to the value of the second ; and we have, as our final approximate result,

$$\mathrm{F} = \tfrac{1}{4} g h^2 \left(1 - \frac{4 m \mathrm{D}}{\epsilon^{2m\mathrm{D}} - \epsilon^{-2m\mathrm{D}}} \right) \quad \cdots \quad \cdots \quad (25).$$

There is no difficulty in understanding the permanent steadiness of the motion which we have now been considering: to any finite distance, however great, on either the up-stream or down-stream side of the inequalities, if the water in the finite space considered is given in this state of motion, and if water is admitted on the one side and carried away on the the other side conformably. But it is very interesting and instructive to consider the initiation of such a state of things from an antecedent condition of uniform flow over a plane bottom. Suppose, as the primary condition, an inequality, whether elevation or depression, to exist in the bottom, but to be carried along with the water, so that the flow of the water is everywhere uniform and in parallel lines. If the inequality is an elevation above the bottom, our supposition is that the whole projecting piece, moving with the water, slips along the bottom. If the inequality be a depression in the bottom, the more awkward supposition must be made of a plasticity of the bottom, and the form of the inequality carried along, while the bottom is kept rigidly plane before and after this depression.

Suppose, now, the inequality is gradually or suddenly brought to rest, what will be the resulting motion of the water ? The question is identical with that of finding the motion of water in a canal, when by an external force, such as that of a towing-rope, a boat is gradually or suddenly set in motion through it ; or, rather, it would be identical if the boat were a beam filling the whole breadth across the canal, so that the motion of the water shall be purely two-dimensional. I hope in a later article (Part III. or Part IV. of the present series) to investigate the formation of the procession of standing waves in the wake of the obstacle, and its gradual extension farther and farther down-stream from the obstacle, the motion having become sensibly steady in the its neighbourhood, and becoming so to greater and greater distances down-stream by the completion of the growth of fresh waves. The disturbance sent up-stream from the initiating irregularity must also be considered. Equation (15) shows that whether the irregularity be an elevation, as in our first diagram (fig. 1), or a depression, as in fig. 2, a rising of level must travel up-stream, at a velocity relatively to the water which we know must be $\sqrt{g \mathrm{D}_0'}$, where D_0' is inter-

mediate between D_0 and the smaller depth, which we shall call D', in the undisturbed stream above. But however gradually the initiating irregularity may have been instituted, this travelling of an elevation up-stream must develop a bore; because the velocity of propagation is, as it were, different in different parts of the slope, being $\sqrt{gD'}$ at the commencement of the slope, and ranging from this, through $\sqrt{gD_0'}$, to $\sqrt{gD_0}$ as the depth rises from D' to D_0; so that, as it were, the brow of the plateau in its advance up-stream overtakes the talus, till the slope becomes too steep for our approximation. The inevitable bore and " broken " water (inevitable without viscidity of the water, or some surface-action preventing the excessive steepness) would modify affairs down-stream in a manner which it is difficult to imagine. It becomes, therefore, interesting to see how it may be avoided, whether by surface-action, or by giving some viscosity to the water. It is more interesting to do this by surface-action, and to allow the water to be perfectly inviscid, so that our standing waves down-stream may be perfectly unimpaired. And we may do it very simply by covering the free surface all over (up-stream and down-stream) with an infinitely thin viscously elastic flexible membrane, stiffened transversely (after the manner of the sail of a Chinese junk) by rigid massless bars with ends travelling up and down in vertical guides on the sides of the canal. If we suppose the motion of these ends to be resisted by forces proportional to their velocities, and the membrane to exercise (positive or negative) contractile tensional force in simple proportion to the velocity of the change of its length in each infinitely small part ; we have a mechanical arrangement by which is realized the mathematical condition of a surface normal pressure varying according to normal component velocity of the otherwise free surface, and in simple proportion to this normal velocity when the slope is infinitesimal. By making the viscous forces sufficiently great, we may make the progress of the rise of level up-stream as gradual as we please, and perfectly avoid the bore. We may also make the progress of the procession of stationary waves down-stream as slow as we please. The form of the water-surface over the inequality or inequalities, and to any distance from them, both up-stream and down-stream, is not ultimately affected at all by the viscous covering ; and it becomes, as time advances, more and more nearly that of the mathematical solution for steady motion, which I hope to give, with graphic illustrations drawn according to calculation from the solution, in Part III.

LIII. *New Geometrical Representation of Moments and Products of Inertia in a Plane Section; and also of the Relations between Stresses and Strains in two Dimensions. By* ALFRED LODGE, *M.A., Coopers Hill, Staines* [*].

THE object of the first part of this paper is to give two methods by which the connection between the moments and products of inertia about pairs of rectangular axes through a point may be represented by means of a circle, without the necessity of drawing the ellipse of inertia.

If a, b are the radii of gyration about the principal axes OA, OB at the given point O, and k, l those about any other pair of rectangular axes through the same point, h being the product-coefficient about the same pair (*i.e.* the product of inertia divided by the area of the section), we have

$$k^2 = a^2 \cos^2\theta + b^2 \sin^2\theta, \quad \ldots \quad (1)$$

$$l^2 = a^2 \sin^2\theta + b^2 \cos^2\theta, \quad \ldots \quad (2)$$

$$h = (a^2 - b^2) \sin\theta \cos\theta, \quad \ldots \quad (3)$$

where θ is the angle KOA, considered positive when measured from $+$OA towards $+$OB, and when the right angle from $+$OK to $+$OL is measured in this positive direction.

First Method of Geometrically representing the above Relations.

With diameter equal to $a+b$ describe a circle passing through O, but otherwise in any position whatever in the plane of the section, cutting the principal axes OA, OB in A, B respectively. This may be called the gyration-circle at O.

On the diameter AB of the circle take a point P, such that PA $= a$, and PB $= b$.

Then, if OK, OL are a pair of rectangular axes, cutting the circle in K, L respectively,

Fig. 1.

PK is the radius of gyration about OK;

PL is the radius of gyration about OL;
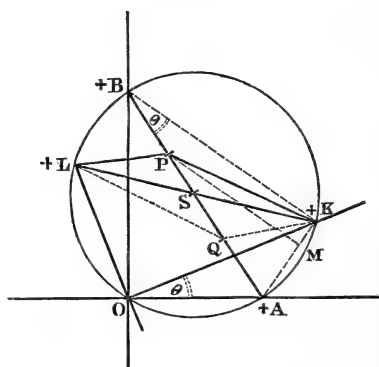
and twice the triangle KPL is the product-coefficient about the pair OK, OL.

For, draw PM perpendicular to AK ; then $PM = a \cos \theta$, $MK = b \sin \theta$; hence

$$PK^2 = a^2 \cos^2 \theta + b^2 \sin^2 \theta = k^2.$$

Similarly, $PL^2 = l^2.$

Also LK (the base of the triangle KPL) $= a + b$; and, if S is the centre of the circle, $SP = \frac{1}{2}(a-b)$, and the angle $KSA = 2\theta$, therefore the height of the triangle

$$= \tfrac{1}{2}(a-b) \sin 2\theta = (a-b) \sin \theta \cos \theta ;$$

∴ twice area of triangle $KPL = (a^2 - b^2) \sin \theta \cos \theta = h.$
$$\text{Q. E. D.}$$

The *sign* of the product h is positive if the positive directions of OK, OL include between them the axis of minimum moment (as in the figure), for in that case θ is positive and less than a right angle, and $a^2 - b^2$ is positive ; *or* θ is negative and less than a right angle, and $a^2 - b^2$ is negative. This condition is the same as the following :—If the centre S is in the positive quadrant KOL, the product of inertia is positive if O, P are on opposite sides of KSL. If S is taken *on* one of the axes OK, OL, when the product is positive P is in the positive quadrant.

If Q be taken on AB so that $SQ = SP$, the figure PKQL is a parallelogram with sides equal to the radii of gyration about O K, OL, whose area equals the product-coefficient about OK, OL, and one of whose diagonals is the *sum* of the principal radii of gyration, the other being the *difference* ; P, Q being the ends of the shorter diagonal.

Hence, if the radii PK, PL, and the product-coefficient about OK, OL are given, it is easy to find the radius SK of the gyration-circle at O, and to construct for the principal axes.

It is worth noticing that if PK is drawn perpendicular to OK, LQ lies along LO, and P is on the positive or negative side of LO, according as the product of inertia is positive or negative.

It is not difficult, being given the position of the centre of area G, to construct for the central principal axes. For, suppose G lies on OK ; draw GL' parallel to OL. Then the radius about GK is known, the product of inertia about GK, GL' is equal to that about OK, OL, and they are both therefore represented by parallelograms of the same height, and with one pair of sides of the same length. The other sides of the new parallelogram are of length equal to $\sqrt{PL^2 - OG^2}$,

and are therefore easily constructed. This completes the parallelogram, giving all data for the gyration-circle belonging to G.

Second Method of representing the Relations (1), (2), *and* (3).

Let UA=the moment of inertia about OA, and UB=AV= the moment about OB, OA and OB being, as before, the principal axes at O : so that UV is the *sum* of the principal moments, and AB their *difference*.
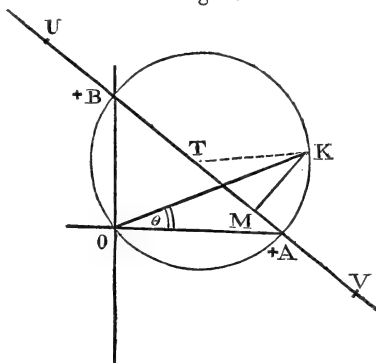
Describe the circle AOB with centre T. Let any axis OK cut the circle in K, and from K draw KM perpendicular to UV.

Fig. 2.

Then UM is the moment of inertia about OK,

VM is the moment about OL (perpendicular to OK),

and KM is the product of inertia about OK, OL.

For, taking the area of the section as unity, which does not affect the relations between the quantities,

$$UM = UT + TM = \tfrac{1}{2}(a^2 + b^2) + \tfrac{1}{2}(a^2 - b^2) \cos 2\theta,$$
$$= a^2 \cos^2 \theta + b^2 \sin^2 \theta = k^2 \;;$$
$$VM = VT - TM = a^2 \sin^2 \theta + b^2 \cos^2 \theta = l^2 \;;$$
$$KM = TK \sin 2\theta = \tfrac{1}{2}(a^2 - b^2) \sin 2\theta = h.$$

These relations hold in whatever position the circle OAB is, and therefore O may be in the same straight line with MK, in which case OM=MK. Hence, if the moments and product about OK, OL are given, we have the following construction for the principal axes :—

On OK take OM=the given product of inertia, measuring OM in the positive or negative direction according as the product is positive or negative. From M draw MU parallel to OL in the positive direction equal to the moment about OK, and in the opposite direction draw MV equal to the moment about OL, so that UV is the sum of the moments. Bisect UV in T, and with centre T, radius TO, describe a circle cutting UV in A and B. Then OA, OB are the principal axes, and UA, UB the moments about them.

2 I 2

The proof is the converse of the preceding proof. The only thing requiring special consideration is whether the axis of minimum moment will fall in the proper quadrant KOL, *i. e.* in the positive quadrant if OM is positive, and out of it if OM is negative.

The axis of minimum moment will evidently be in the same quadrant as the point U from which the moments are measured, and by the construction U will be in the first quadrant if OM is positive and in the second if OM is negative ; which completes the proof.

Fig 3.

The expressions for the principal moments in terms of the given moments and product are easily deduced from the figure, viz.:—

$$a^2, b^2 = \mathrm{UT} \pm \mathrm{TA} = \mathrm{UT} \pm \sqrt{\mathrm{TM}^2 + \mathrm{MK}^2}$$
$$= \tfrac{1}{2}(k^2 + l^2) \pm \tfrac{1}{2} \sqrt{(k^2 - l^2)^2 + 4h^2}, \quad . \quad . \quad . \quad . \quad (4)$$

and the angle AOK (θ) is given by the equation

$$\tan 2\theta = \tan \mathrm{KTA} = \frac{2h}{k^2 - l^2}. \quad . \quad . \quad . \quad (5)$$

These equations are of course deducible from the relations (1), (2), and (3).

Stresses and Strains.

The above construction is also very useful for the graphic determination of principal stresses and principal strains from given two-dimensional stress or strain conditions in any two rectangular directions, as the equations are exactly of the same form as (4) and (5).

Thus, let OX, OY be two given rectangular directions, and let p_1 be the normal component and q the tangential component of a given stress on planes perpendicular to OY, and p_2, q the normal and tangential components of a given stress on planes perpendicular to OX, the whole action being restricted to the plane XOY; and let p_1, p_2 be considered positive when tensile, and q positive when it tends to produce positive sliding, *i. e.* when it tends to *diminish* the angle between $+$OX and $+$OY.

On OX take $OM = q$ in magnitude and direction. From M draw $MU = p_1$ in magnitude and direction (*i.e.* upwards if tensile, downwards if compressive), and on UM take $MV = p_2$

Fig. 4.
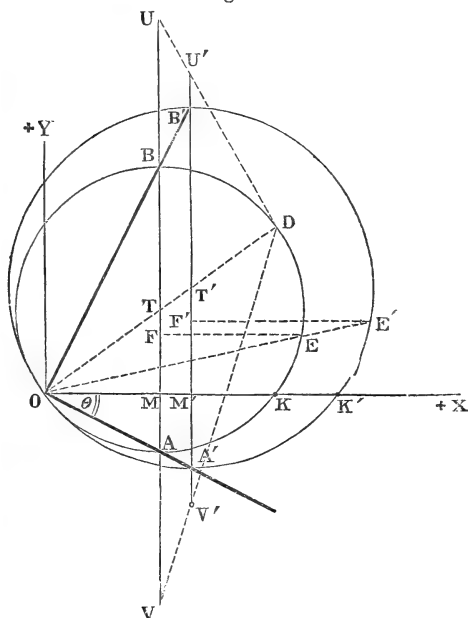
in such way that $UV = p_1 + p_2$. [In the figure p_1 and p_2 are both tensile, and q is positive.]

Bisect UV in T, and with centre T and radius TO describe a circle cutting UV in A, B.

Then OA, OB are the directions of the principal planes, and UA, UB the corresponding principal stresses (p, p').

For, if $AOK = \theta$, $\tan 2\theta = \dfrac{2q}{p_1 - p_2};$

and

$$UA, \ UB = \tfrac{1}{2}(p_1 + p_2) \pm \tfrac{1}{2}\sqrt{(p_1 - p_2)^2 + 4q^2},$$

which are the equations for the directions and magnitudes of the principal stresses.

The principal strains i, i' can also be shown by an extension of the same figure; for if E is Young's modulus for the material, and η the ratio of lateral compression to longitudinal strain for a single stress, the following relations hold:—

$$E(i + i') = (1 - \eta)(p + p');$$
$$E(i - i') = (1 + \eta)(p - p').$$

Hence, if on the diameter OTD a point T′ be taken such that OT′=$(1+\eta)$OT, and if through T′ a parallel to AB is drawn cutting OA, OB in A′, B′ respectively and DU, DV in U′, V′ respectively, we have U′V′=2U′T′=$(1-\eta)(p+p')$, and A′B′=2A′T′=$(1+\eta)(p-p')$; so that U′A′=Ei, and U′B′=Ei'.

Also, if with T′ as centre and T′O as radius a circle be drawn, it will pass through A′, B′, and will give the strains in any directions, in the same way as the circle OAB gives the stresses. For example, the stress on the plane OEE′ has the normal component UF, and the tangential component FE ; while the normal strain is proportional to U′F′ and the sliding (g) to 2F′E′.

The figure also shows the relation between E and the coefficient (G) of elasticity of sliding ; for FE = Gg, and 2F′E′=Eg, therefore

$$E : G = 2F'E' : FE = 2(1+\eta).$$

Coopers Hill,
    October 11, 1886.

LIV. *On the Distinction between Spectral Lines of Solar and Terrestrial Origin.* By Prof. M. A. Cornu[*].

[Plate VIII.]

FRAUNHOFER, when he discovered the dark lines with which the solar spectrum is crossed, gave them names in order to facilitate description ; the principal lines were designated by the letters A, B, ... H in such a way as to separate approximately the seven principal colours of the spectrum. The subsequent observations of Brewster, Dr. Gladstone, and M. Janssen proved that, notwithstanding the symmetry of denomination and the identity of appearance, these dark lines belonged to two distinct classes. Indeed, the one preserves always the same aspect, while the other becomes broader and darker as the sun approaches the horizon.

The first, most of which have been identified with the bright lines due to metallic vapours (iron, magnesium, calcium, nickel, &c.), have been attributed, since the researches of Prof. Kirchhoff, to the absorption produced by metallic substances in a state of vapour on the surface of the sun. The other, in consequence of their intensity varying with the thickness of the atmosphere traversed by the sun's rays, are explained by the selective absorption due to the cold gases or

vapours of the earth's atmosphere. We ought then to distinguish the *solar* from the *telluric* lines.

Thus, of the eight principal Fraunhofer-lines, six are characteristic of metallic elements and are of solar origin (C and F, hydrogen ; D, sodium ; E, G, iron ; H, calcium) ; the other two (A and B) are telluric.

Fraunhofer had besides distinguished two complex groups, namely a band *a*, very broad, in the extreme red, and a well-marked triple line, *b*, in the green ; *b* is solar (magnesium), and *a* of terrestrial origin (fig. 1). Brewster, on discovering

Fig. 1.
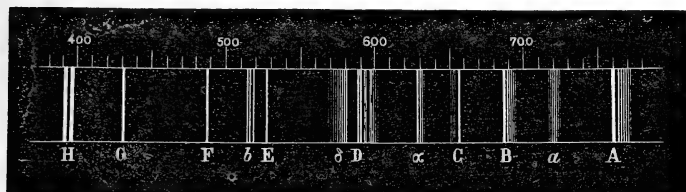
Solar spectrum, with principal lines marked.

new bands of variable intensity in the spectrum, added new designations ; it is sufficient here to mention the band *α* situated in the orange, and the band *δ* in the yellow. These symbols have been adopted by Ångström (*Spectre normal du Soleil*).

Up to the present time it has been considered a difficult, and in any case a troublesome, matter to distinguish between these two kinds of lines. It was necessary, in fact, to observe the solar spectrum at two very different altitudes of the sun, under various meteorological conditions, to be able to affirm that the spectrum-lines do or do not change in intensity with the thickness of the atmosphere or the quantity of water-vapour traversed by the solar rays.

The improvement of spectroscopes, in respect of the sharpness and especially of the dispersion of the lines, has allowed me to arrive at a method which renders the distinction between the two kinds of rays in a certain sense *intuitive*.

This method is founded on a principle due to M. Fizeau*—the principle of the displacement of the spectral lines of the light emitted by a source which is in absolute or relative motion. We easily obtain the expression for the apparent wave-length λ' of a radiation from a point in motion with a

* *Bulletin de la Société Philomathique*, décembre 1848 ; and *Ann. de Chim. et de Phys.* 4 série, t. xix. p. 211.

relative velocity v, the true wave-length being λ, viz. :—

$$\lambda' = \lambda\left(1 - \frac{v}{V}\right),$$

This principle may be applied directly to the light emitted by the solar disk at the two extremities of an equatorial diameter.

The absolute velocity v of a point on the solar equator is very sensibly 2 kilometres per second, that of the velocity of light, V, 300,000 kilometres per second. Hence we shall have a variation of wave-length equal to

$$\Delta\lambda = \pm\lambda \times \frac{2}{300,000} = \pm\frac{\lambda}{150,000},$$

+ or — according as we take the same radiation at the eastern or western end of the solar equator.

If we wish to know numerically the magnitude of the displacement of a spectral line corresponding to this variation of wave-length, it is sufficient to substitute for λ the numerical value which defines the region which we wish to observe. Consider, for example, the two D lines ($\lambda_1 = 588\cdot40, \lambda_2 = 588\cdot89$). The displacement of a line having $\lambda = 589$ will be

$$\Delta\lambda = \pm\frac{589}{150,000}.$$

If we wish to compare this displacement with the distance between the two D lines (*i. e.* with $\lambda_2 - \lambda_1 = 0\cdot49$), we shall have as the relative displacement,

$$\frac{\Delta\lambda}{\lambda_2 - \lambda_1} = \pm\frac{589}{\cdot49 \times 150,000} = \frac{1}{124\cdot8}.$$

The double displacement will therefore be $\dfrac{2}{124\cdot8}$ or $\dfrac{1}{62\cdot4}$ of the distance between the two D lines.

This total displacement, small as it is, is perfectly appreciable with spectroscopes of very high dispersion, such as the prism-spectroscope of M. Thollon, and the instruments with diffraction-gratings constructed by Prof. Rowland of the University of Baltimore.

With the magnificent grating presented by Prof. Rowland to the École Polytechnique, the double displacement may become sensible almost with all the points of the solar contour ; that is to say, even with those which are far from giving the maximum separation.

The experimental method consists in causing the images of the two extremities of the solar equator to fall alternately on the slit of the spectroscope. For this purpose the solar beam is received on a condensing-lens, which produces in the plane of the slit a sharp image of the solar disk. The substitution

of one of the extremities for the other produces the double displacement of the solar radiations, while it has no influence upon the position of the absorption-lines of the atmosphere.

The analysis of the optical conditions shows that perfect sharpness of the displacement can only be obtained when three experimental conditions are fulfilled :—

1. The spectral images at the focus of the telescope of the spectroscope must be *aplanatic* ; that is, such that the vertical lines (spectral lines) and the horizontal lines (due to the imperfections of the slit) are equally sharp in the same plane.

2. This plane must coincide accurately with that of the cross wires.

3. The focal image of the solar disk must be exactly in the plane of the slit of the collimator.

The omission of any one of these adjustments would involve an abnormal displacement of the lines consequent upon a displacement of the collecting-lens. On the other hand, when all these adjustments have been made the dark lines of terrestrial origin are perfectly fixed, while those of solar origin move with extreme sharpness.

If we take as a fixed mark a dust particle on one of the cross wires, such as are always to be found on wires, we are able to distinguish immediately, *at a glance*, solar from telluric rays.

Finally, I may add that the displacement becomes still more sensible when the substitution of one solar edge for the other is effected rhythmically. This is done by causing the collecting-lens to oscillate two or three times per second *.

* The following (see fig. 2) is the arrangement employed :—The collecting-lens C is carried by a socket S S′ which rests on the table U U′ of

Fig. 2.
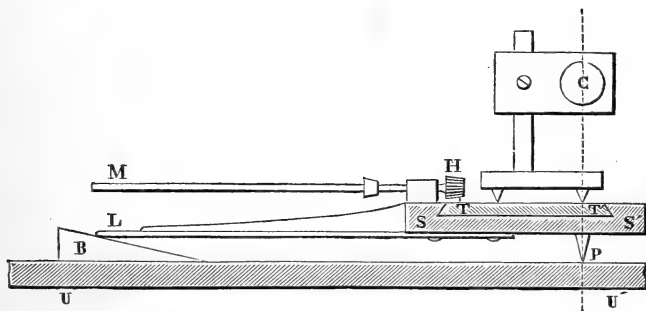
the spectroscope by two points P (one lies behind the other in the figure), about which it can turn. The optic centre C of the collecting-lens describes a small horizontal element, and transmits the same motion to the image of the solar disk. The observer produces this motion by the aid of the lever L. The pinion H serves to put the collecting-lens exactly in focus on the slit.

The eye then becomes sensitive to the least oscillation of the lines.

The distinguishing of the lines becomes then entirely intuitive. We observe each spectral line, and the result is such as would be produced by shaking it. If it remains immovable, it is a telluric line ; if it oscillates, it is a solar line.

The study of the telluric lines of the solar spectrum becomes therefore infinitely easier than heretofore. I have devoted to this study the fine days of the last few years, and the results have been very fruitful. Amongst the most interesting of the observations I may mention :— (1) the *anatomy* of Ångström's group α (see Plate VIII.), in which I have succeeded in detecting a group possessing the same constitution as the bands A and B, according to the beautiful observation of Prof. Langley ; (2) the telluric nature of a certain number of lines beyond the band δ ; and (3) the solar origin of the line 1474 of Kirchhoff : this line is double under strong dispersion, and as it oscillates we may conclude that the vapour which absorbs the radiations of which it takes the place is carried round by the rotation of the sun *.

* The oscillation of the lines is not the only means of distinguishing the two kinds of lines. If (by means of a Wollaston's double refracting-prism) we obtain two images of the solar disk in such a way that the two opposite extremities of the equatorial diameter are tangent and normal to the slit (fig. 3), the telluric lines T T′ of the spectra of the two images

Fig. 3.
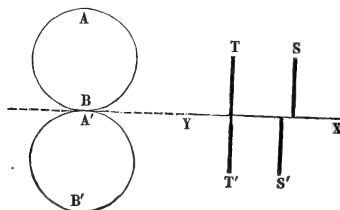
Fig. 4.
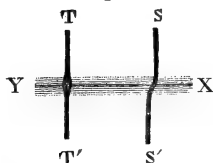
Fig. 5.
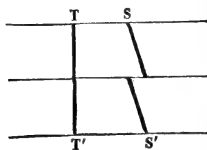
are upon the same line, while the solar lines S S′ are dislocated. This dislocation is very sharp if the adjustments enumerated above have been properly carried out; otherwise we obtain the confused appearance of fig. 4. If we employ a collecting-lens of very short focus, giving a very small image of the solar disk, it is easy to show that the appearance of

To sum up:—Physicists and astronomers have now in their hands an extremely useful method for studying the constitution of the solar spectrum, a method which enables us to distinguish at a glance between lines of solar and terrestrial origin.

LV. *Notices respecting New Books.*

The Volcanoes of Japan. By JOHN MILNE. *Transactions of the Seismological Society of Japan*, Vol. IX. Part II. 8vo. Yokohama, 1886: pp. 184, with a map and numerous sketches.

MR. JOHN MILNE, F.G.S., occupies the whole of this Part of the Transactions with another of his important papers on the Volcanic Phenomena of Japan. Rich as Japan is in the possession of Volcanoes, there does not appear to have been any native author who has endeavoured to describe in full these conspicuous landmarks. From the limited bibliography of native works on the subject (avowedly imperfect, and no doubt difficult to collect) given in this Memoir, we can only note one which, from its title, would appear to be a definite treatise, namely no. 26, *Kyūshū Kwazan Ron*, by Aoe Shin, giving an account of the Volcanoes in Kyūshū. Of European writers, Naumann, Von Drasche, and D. H. Marshall have contributed to our scanty knowledge; but it has been left for John Milne, in addition to his many notes on the subject, to supply us with this systematic and exhaustive work on the " more important Volcanoes of Japan."

After the bibliography, which consists of 42 references, and which would have been more valuable had the dates been given, Mr. Milne proceeds at once to take the Volcanoes individually, on a plan of his own, not easy to follow and apparently with no definite arrangement. A minute account of the date and extent of each recorded eruption is given, together with many curious and interesting particulars as to the religious signification attached thereto, and the folk-lore concerning the mountain and its outbursts. This is followed by a geological description, and accompanied by an outline-sketch of the volcano, generally from a photograph.

He finishes this part of the Memoir with a table of all the known eruptions, showing a comparison of the activity in summer and winter months. The Japanese measurements, often used here, should have been translated or reduced to their English equivalents, in a table, or throughout, instead of in the rare and scattered instances met with.

One hundred and sixty-two pages are devoted to these important points ; and Mr. Milne then goes on to give his conclusions, from personal observation, that the two islands, Iturup and Kunashiri, are older than any other members of the Japanese

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the figure (3) is modified. The telluric lines remain vertical, while the solar lines are not only dislocated, but become oblique (see fig. 5). We have here a very delicate test which lends itself to photographic observations.

group, and that they "form the first of a series of stepping-stones which connect Japan, by means of Kamschatka, with the remainder of Asia."

A few observations on the slope of the cones follow; and Mr. Milne records one instance, that of the "small cone rising from the upper crater of Cha-cha-Nobori," in which the inclination is 37°. A reference to another cone—Atatsu-Nobori, at p. 147,—"one side having a slope of 50°, and the other of 49°"—might have been repeated here. Mr. Milne says that Atatsu-Nobori is "the steepest volcanic cone I have yet seen."

Of the Kuriles, Mr. Milne writes, "They are, so to speak, amongst the last of the links which together build up the Volcanic chain which bounds the shores of the West Pacific," and "they are probably contemporaneous with the younger volcanoes of Kamschatka and Japan."

The author lays great stress on the importance of the Japanese volcanoes as land-builders, and refers the low altitude of the Kuriles, as compared with the Volcanic Peaks of Klutchewsk (16,500 ft.) in Kamschatka, and of Fuji-yama (12,450 ft.), to the fact that they were "probably built up from the bottom of an ocean which is perhaps the deepest in the world." The rocks that he and his assistants collected appear to be augitic andesites. The absence of lava-streams is noticed, and pointed out as "suggestive of the way in which these islands have been built up"—by cindery accumulations or ash-beds. One of the most important parts of the Memoir is the map of the Volcanoes of Japan, in which the exact positions of one hundred and twenty-nine active or extinct craters are indicated. In a table accompanying it, a note is given of the height, and the nature of rock, with general remarks concerning each volcano. Of the 129, fifty-one are still active. Thirty-nine have symmetrically formed cones.

The paper concludes with well-considered remarks on the Relative Age of all of them, the characters of their Lavas and other Rocks, the intensity of Eruptions, and the general form of the Volcanoes. To assist the student in following the author on the last-mentioned subject he has reproduced the diagrammatic plate from the 'Geological Magazine' for 1878, and added a plate of various profiles of Fuji-san ("called by foreigners Fuji-yama") from photographs and surveys of the mountain; and he quotes an interesting series of causes which help to modify the natural curvature of a volcano. Some important notes also are given as to the height of Fuji-san; and the conclusion Mr. Milne arrives at is that the proper height should be taken as between 12,400 and 12,450 feet. The memoir is profusely illustrated with lithograph sketches, chiefly exact outlines of the various cones referred to.

Discussions on Climate and Cosmology. By JAMES CROLL, *LL.D.*, *F.R.S.* Edinburgh: Adam and Charles Black. 1885.

IT is now nearly a quarter of a century since Dr. Croll first enunciated the outline of the theory of the cause of glacial phenomena,

a problem which previous to that time had engaged and perplexed many minds. Till Dr. Croll's famous theory appeared in the pages of this Magazine, no hypothesis had been propounded which satisfied the conditions of the problem even as known at the time ; while the various tentative explanations for the most part demanded the calling into existence of agencies and of terrestrial changes which would themselves be more difficult to account for than the phenomena they were meant to explain. It was from the first seen and conceded that the varying eccentricity of the ecliptic, upon which Dr. Croll bases his theory, has a real existence in nature ; and the only points then, and now, in dispute have relation to the manner and extent to which these cosmical causes affect terrestrial climate and conditions. Early in the century, Sir John Herschel, Arago, and others had given attention to the question of the relation of eccentricity to climate ; and they came to the conclusion that it neither affects the amount of heat received from the earth nor, to any appreciable extent, its distribution.

Dr. Croll thus brought out his theory in the face of, and against, the confidently expressed opinion of some of the greatest authorities in physical science. The door which he sought to open had been officially barred for a long generation, and when he pushed through it he found no beaten path on the other side. Notwithstanding all that had been written about the Glacial Epoch, the facts and phenomena of the period were but imperfectly stated and understood, and the science of Climatology was either non-existent or in the most chaotic condition. Gradually and laboriously he built up his theory ; he accumulated facts and observations with the most painstaking industry ; and these he marshalled and arranged with consummate skill, till he built up and solidly buttressed one of the most important and far-reaching doctrines which has been enunciated in the whole range of geological science. In dealing with the climatological and other physical facts and bearings of his subject, Dr. Croll had little help from the investigations of those who had gone before him. He had, indeed, to combat many erroneous notions which had become generally entertained from the works of popular writers. Of the great physicists, some had given opinions directly opposed to Dr. Croll's contentions ; others had given no consideration to the climatological relations of terrestrial and cosmical phenomena ; and many were by no means agreed as to the effects of such phenomena. Thus there was scarcely any readymade help available for incorporation with Dr. Croll's work ; he had few witnesses to call in his favour ; he had many stumblingblocks to remove, much to explain away, and much to argue against. With indomitable patience and perseverance he set himself to his task ; with calmness and temperance, and yet with marvellous intellectual alertness, he met the arguments of opponents, adhering to his position, and maintaining his view, with modest tenacity and resolution, which commanded the respect and esteem of his most powerful opponents.

The greater part of what Dr. Croll has written on the subject of

geological climate, and the principal controversies which have arisen in connection with the theory, have appeared in the pages of this Magazine, and may be presumed to be familiar to its readers. His own contributions to the controversy during the last ten years Dr. Croll has now gathered together, and presented to the public in the volume which forms the occasion of this notice. The work is largely controversial in tone, more taken up with maintaining the position of the theory as enunciated in ' Climate and Time ' than in broadening its foundations by new views, or even in resolving difficulties and clearing the logical path along which Dr. Croll leads his adherents. Dr. Croll is not prepared to accept any of the modifications of his theory which have been proposed ; still less is he inclined to coincide in the arguments which would be subversive of his position. In the preface to this volume he intimates that he has now spoken his last word in defence of his theory. " There are many of the topics discussed," he says, " which I could have wished to consider more at length ; but advancing years and declining health constrain me to husband my remaining energies for work in a wholly different field of inquiry—work which has never lost for me its fascination, but which has been laid aside for upwards of a quarter of a century." Such a decided intimation of withdrawal from a controversy in which he has so long been the principal figure surely forms a fitting occasion for an acknowledgment of the vast and varied services Dr. Croll has rendered to an obscure branch of science, of the amount of intellectual activity and industry to which his writings have given rise, of the large measure of light he has thrown on what appeared to be the most complex and puzzling phenomenon which meets the student at the very gateway of geological investigation, and to the stimulating and suggestive models he has afforded for dealing with the ravelled skein of geological history in many of its departments.

Dr. Croll's theory, all opposition notwithstanding, holds the field. Many have taken objection to the entire fabric, and still more have been opposed to various propositions by which the theory is supported ; but no opponent has been able to suggest that most powerful of all arguments—a counter-theory. It is almost a melancholy sight to see the learned President of the British Association falling back upon the antiquated Lyellian doctrine. Whatever may satisfy the conditions of the problem of Glacial periods, it is now well known that Sir Charles Lyell's theory of polar continents and tropical oceans will not supply the key ; but Principal Dawson can discover, or has at his service, no other. But although no effective counter-theory has been started, even as a stalking-horse, against the Croll doctrine, it is not to be concluded that the controversy is at an end, and that henceforth the theory that periodic changes of climate are primarily caused by Eccentricity is to be accepted as an article of faith in the schools of geological and physical orthodoxy. It would indeed be a misfortune were the controversy, which has gone on so briskly for about twenty-five years, to be allowed to subside. Authorities who are prepared to accept

the cardinal points of the doctrine are by no means agreed as to many of the assumptions on which it is based, nor as to the issues of the argument for the case. The whole subject is comparatively new, and the men who are entitled to express an opinion on it are not numerous. Ere a general concensus of opinion can be arrived at much investigation and debate are necessary; foot by foot solid ground must be conquered, and it will be found, as truth emerges, that it will illumine, not only much that is yet obscure in the theory of climatic perturbations, but that, like all truth, it will become the fertile mother of a long line of exact knowledge.

LVI. *Intelligence and Miscellaneous Articles.*

ON THE MAGNETIC ROTATION OF MIXTURES OF WATER WITH SOME OF THE ACIDS OF THE FATTY SERIES, WITH ALCOHOL, AND WITH SULPHURIC ACID; AND OBSERVATIONS ON WATER OF CRYSTALLIZATION [*]. BY W. H. PERKIN, PH.D., F.R.S.

FROM the Author's previous work on the magnetic rotation of compounds it was found that the molecular magnetic rotation of water, which is taken as unity, is not the same as the sum of the values of oxygen and two of hydrogen, as deduced from the molecular magnetic rotation of other bodies. Thus hydrogen is found to be 0·254, whilst hydrogen in hydroxyl varies from 0·194 in ordinary alcohol to 0·137 in monobasic acids, and is 0·261 in carbonyl; so that, taking the lowest number, it gives $H_2 + O = 0·645$, and taking the highest it is 0·769.

From these facts it appeared that the determination of the magnetic relation of hydrated bodies might give numbers which would show whether they contained water or whether the substances with which it was mixed had combined so as to form new compounds. If the former were the case the molecular rotation should represent that of the compound + that of water; if the latter, it should be lower than this. For example, if formic acid were mixed with water, molecular proportions being used, either $H.COOH + H_2O$ or $H.C(HO_3)$ might be produced. In the first case the rotation should be

$$\begin{array}{ll}
\text{Formic acid} & 1\cdot671 \\
\text{Water} & 1\cdot000 \\
\hline
& 2\cdot671
\end{array}$$

In the second, taking the highest value of $H_2 + O$, it would be

$$\begin{array}{ll}
\text{Formic acid} & 1\cdot671 \\
\text{Water} & 0\cdot769 \\
\hline
& 2\cdot340
\end{array}$$

which is considerably lower.

[*] Discussion on the "Nature of Solution," British Association, Birmingham Meeting, Section B.

As some of the fatty acids have been believed to unite with water, to form trihydric alcohols, they were elected for examination; and at the same time hydrated alcohol was also examined, because it could not form a compound with water, and would therefore act as a check upon the results.

The results of the examination show that formic, acetic, and propionic acids, when mixed with water, do not form new compounds, but that the products simply consist of these bodies and water.

Sulphuric acid in the pure and hydrated conditions was next examined, viz. H_2SO_4, $H_2SO_4 + H_2O$, $H_2SO_4 + 2H_2O$, and $H_2SO_4 + 3H_2O$. The numbers obtained in this case show that combination takes place chiefly when one molecule of water has been added, only to a small extent in the case of the second addition, and scarcely at all when the third is added; and the author gives reasons for considering that sulphuric acid combines with one molecule of water only, forming the compound $(HO)_4SO$.

Whilst studying the nature of the hydrated products, the author's attention was drawn to the subject of water of crystallization; and from the inconsistency as to the presence or absence of water of crystallization in compounds of the same class—as, for example, in those of silver, potassium, and sodium, and also methyl bromide, as compared with analogous compounds—he considers it impossible to believe that water of crystallization has any relationship to chemical combination; that of course refers to water when it exists as such, and not to hydrogen and oxygen present in the proportions found in water but otherwise combined. If this be so, it is thought that its association with chemical compounds is most likely connected with the building-up of the crystalline form, it being difficult to see what other part it can play; and the reason why some compounds crystallize without and some with water of crystallization is probably determined by the tendency to produce that form which can be the most readily built up : if that form can result from the anhydrous salt, anhydrous crystals are formed; if with the salt and water, then the crystals will contain water of crystallization; and it is well known how change of conditions will cause variation in the proportions of water of crystallization, and also form of crystal.

These observations would also apply to compounds crystallizing with alcohol, acetic acid, benzene, &c., and to some double salts, where one or more of the constituents would act like water of crystallization. Attention is drawn to the fact that if the above view of water of crystallization be correct, it is evident that a salt containing water of crystallization will be resolved into water and the salt on breaking up of the crystalline form by solution, which is believed to be the case by many who have studied the subject of solution.—*The Chemical News*, Oct. 22, 1886.

LVII. *Notes on Electricity and Magnetism.*—II. *The Self-induction and Resistance of Compound Conductors.* *By* Lord RAYLEIGH, *Sec. R.S., D.C.L.*[*]

BEFORE entering upon the proper subject of this communication, I take the earliest opportunity of correcting a mistake of some importance into which I fell in connection with induction-coils. From the fact that a closed magnetic circuit acts very ill as a store of energy, I argued that closed electromagnets were to be condemned as secondary generators (Phil. Mag. August 1886, p. 179). Dr. Hopkinson and Prof. Ewing have independently drawn my attention to the fact that the success of induction-coils does not depend upon storage of energy, and that they might work with high efficiency even though the whole of the energy put into the iron were wasted. Prof. Ewing's remark, to which I took exception, is thus perfectly correct.

In his inaugural address to the Society of Telegraph Engineers[†], and in a subsequent communication to the Royal Society[‡], Prof. Hughes has described a series of interesting experiments, which have attracted a good deal of attention in consequence both of the official position and known experimental skill of the author. Some of the conclusions which he advances can hardly be sustained, and have met with severe criticism at the hands of Weber, Heaviside, and others. There

[*] Communicated by the Author.
[†] Journ. Tel. Eng. vol. xv. (1866) p. 1.
[‡] Proc. Roy. Soc. vol. xl. (1886) p. 451.

are certain other points raised by him, or suggested by his work, which seem worthy of consideration ; and I propose in the present paper to give an account of some investigations, mainly experimental, carried on during the summer months, which may, I hope, tend to settle some controverted questions.

Prof. Hughes's first apparatus consists of a Wheatstone's quadrilateral, with a telephone in the bridge, one of the sides of the quadrilateral being the wire or coil under examination, and the other three being the parts into which a single German-silver wire is divided by two sliding contacts. If the battery-branch be closed, and a suitable interrupter be introduced into the telephone-branch, balance may be obtained by shifting the contacts. *Provided that the interrupter introduces no electromotive force of its own**, the balance indicates the proportionality of the four resistances. If P be the unknown resistance of the conductor under test, Q, R the resistances of the adjacent parts of the divided wire, S that of the opposite part (between the sliding contacts), then, by the ordinary rule, PS = QR ; while Q, R, S are subject to the relation

$$Q + R + S = W,$$

W being a constant. If now the interrupter be transferred from the telephone to the battery-branch, the balance is usually disturbed on account of induction, and cannot be restored by any mere shifting of the contacts. In order to compensate the induction, another influence of the same kind must be introduced. It is here that the peculiarity of the apparatus lies. A coil is inserted in the battery and another in the telephone-branch, which act inductively upon one another, and are so mounted that the effect may be readily varied. The two coils may be concentric and relatively movable about the common diameter. In this case the action vanishes when the planes are perpendicular. If one coil be very much smaller than the other, the coefficient of mutual induction M is proportional to the cosine of the angle between the planes. By means of the *two* adjustments, the sliding contact and the rotating coil, it is usually possible to obtain a fair silence.

In his address Prof. Hughes interpreted his observations on the basis of an assumption that the self-induction of P was represented by M, irrespective of resistance, and that the resistance to variable currents could (as in the case of steady currents) be equated to QR/S. In the discussion which followed I pointed out that this was by no means generally true,

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* This condition is not always satisfied. With the reed-interrupter (see below) a loud sound may sometimes be heard, although the battery-branch be open.

and I gave the following formulæ* as applicable to the case in which the only sensible induction among the sides of the quadrilateral is the self-induction L of the conductor P :—

$$QR - SP = p^2 ML, \quad . \quad . \quad . \quad (1)$$

$$M(P + Q + R + S) = SL. \quad . \quad . \quad . \quad (2)$$

The electrical vibrations are here supposed to follow the harmonic law, with frequency $p/2\pi$. " It will be seen that the ordinary resistance balance (SP = QR) is departed from. The change here considered is peculiar to the apparatus, and, so far as its influence is concerned, it does not indicate a rea alteration of resistance in the wire. Moreover, since p is involved, the disturbance depends upon the rapidity of vibration, so that in the case of ordinary mixed sounds silence can be attained only approximately. Again, from the second equation we see that M is not in general a correct measure of the value of L. If, however, P be very small, the desired condition of things is approached; since, by the construction of the apparatus, Q + R + S is constant (say W), and if P be small enough S does not differ much from W, *i. e.* most of the wire forming the three sides of the combination is devoted to the member opposite to P."

The formulæ are easily proved. Since there is no current through the bridge, there must be the same current (x) in P and one of the adjacent sides (say) R, and for a like reason the same current y in the sides Q and S. The difference of potentials at time t between the junction of P and R and the junction of Q and S may be expressed by each of the three following equated quantities :—

$$Qy - Px - L\frac{dx}{dt} = -M\frac{d(x+y)}{dt} = Rx - Sy \,;$$

from which the required results are obtained by elimination of the ratio $x : y$, and introduction of the supposition that all the quantities vary harmonically with frequency $p/2\pi$.

The inadequacy of Prof. Hughes's original interpretation has been remarked upon also by Prof. Weber† and by Mr. O. Heaviside‡, who have obtained the corrected formulæ. I give them here because I agree with Prof. Weber that this form of apparatus possesses distinct advantages. As he points out, if P be known, the application of (2) really presents no difficulty, and allows of L being readily found in terms of M.

* Journ. Tel. Eng. vol. xv. p. 54.
† *El. Rev.*, April 9, 1886; July 9, 1886.
‡ Phil. Mag. August 1886.
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There are many cases in which we may be sure beforehand that P (the effective resistance of the conductor, or combination of conductors, to the variable currents) is the same as if the currents were steady, and then P may be regarded as known.   There are other cases, however,—some of them will be treated below,—in which this assumption cannot be made; and it is impossible to determine the unknown quantities L and P from (2) alone. We may now fall back upon (1). By means of the two equations, P and L can always be found in terms of the other quantities.   But among these is included the frequency of vibration ; so that the method is only practically applicable when the interrupter is such as to give an absolute periodicity.   A scraping contact, otherwise very convenient, is thus excluded*; and this is undoubtedly an objection to the method.

My own experiments have been made with three different forms of apparatus.   The first was constructed upon the model of that originally described by Hughes, and still to be preferred for some purposes.   The others will be described in due course ; but it will be convenient to consider first those parts which are common—the interrupters and the induction-compensators.

The Interrupters.

When regular vibrations are not required, a scraping contact interrupter is the least troublesome.   Mine is of the roughest possible construction.   It is driven by a small jet of water issuing from a glass nozzle in communication with a tap, and impinging upon blades bent in a piece of tin plate and revolving about a vertical axis.   The upper part of the axis carries a small cylinder of roughened iron, against which a brass spring lightly presses. As in Hughes's apparatus, the scraping contact is periodically broken altogether by a projecting finger, which during part of the revolution pushes back the brass spring.   This is a point of some importance, for a faint scraping sound is far better heard and identified when thus rendered intermittent.   The apparatus stands in the sink, so that the water scattered from the revolving blades runs away without giving trouble.   The pressure exercised by the contact-spring requires readjustment from day to day if the loudest sound is wanted.

But for many of the most interesting experiments a scraping contact is unsuitable.   Prof. Hughes has found, indeed, that

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* A toothed-wheel interrupter, as usually employed, does not give a regular vibration of the period corresponding to the passage of a tooth.

in some cases the natural pitch of the telephone-plate is predominant; so that the vibration, as it reaches the ear, is not quite so mixed as might have been expected from its origin. When, however, the induction and resistance under observation are rapidly varying functions of the frequency of vibration, it is evident that no sharp results can be obtained without an interrupter giving a perfectly regular electrical vibration. With proper appliances an absolute silence, or at least one disturbed only by a slight sensation of the octave of the principal tone, can be obtained under circumstances where a scraping contact would admit of no approach to a balance at all.

A thoroughly satisfactory interrupter of this kind has not, to my knowledge, been constructed. Tuning-forks, driven electromagnetically with liquid or solid contacts, answer well so long as the frequency required does not exceed 128 or 256 per second; but here we desire frequencies of from 500 to 2000. My experiments have been made with harmonium-reeds as interrupters, the vibrating tongue making contact once during each period with the slightly rounded end of a brass or iron wire, which can be advanced exactly to the required position by means of a screw cut upon it. Blown with a well regulated wind, such reeds have given good results even up to 2000 (complete) vibrations per second; but they are often capricious and demand frequent readjustment. The reed which I have usually employed makes about 1050 vibrations per second, and answered its purpose fairly well. Hitherto I have not been able to satisfy myself as to the cause of the falling off in efficiency, which often sets in suddenly, and persists until cured by a readjustment. Another objection to this interrupter is the simultaneous production of loud aerial sounds, which must be prevented from reaching the ear of the observer at the telephone by several interposed doors.

The Induction-Compensators.

Two instruments, similar in all respects, were made by my assistant Mr. Gordon, much after the pattern employed by Prof. Hughes. In each there is a small coil mounted so that one diameter coincides with a diameter of a larger coil, and movable about that diameter. The mutual induction M between the two circuits depends upon the position given to the smaller coil, which is read off by a pointer attached to it and moving over a graduated circle. The circles are so divided that the reading (θ) would * be zero when the *axes*

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* The position is mechanically unattainable.

of the coils were coincident, or the planes parallel. In this position M is arithmetically a maximum (M_0); and we consider its algebraic sign to be positive. At 90°, when the axes are at right angles, $M=0$. At 180° M would be negative, and of the same arithmetic value (M_0) as at 0°.

The coils are wound upon boxwood rings, and in each there are 45 convolutions. The mean diameters are about 3 inches and $1\frac{1}{2}$ inch.

Some of the earlier experiments were interpreted by a theory of the compensator, which I knew at the time to be very rough. If the small coil be treated as infinitely small, then

$$M = M_0 \cos \theta.$$

On the same supposition we have, from the roughly measured dimensions,

$$M_0 = 60,000 \text{ centim.}$$

The law of the simple cosine was found to lead to considerable anomalies; and when at a later date (August 19) I carried out my intended calibrations, some very curious results revealed themselves.

The best arrangement for calibration and for determination of the constant of the instruments is to institute distinct primary and secondary circuits. The former included a battery, a scraping contact (p. 472), and the two outer (larger) coils of the compensators. The latter included the two inner coils and a telephone. The precise procedure will depend upon whether we can assume the exact equality of the two compensators. In that case we may introduce, and retain during the observations, another pair of induction-coils, one of course in the primary and the other in the secondary circuit, and of such power as to produce a displacement of about 30° of the compensator. Thus, while in the absence of the additional coils, balance would be obtained when *both* compensators stand at 90°, their introduction would lead to such readings as 90°, 60°; 100°, 70°; 110°, 80°; &c. By this means various parts of the scale of one compensator can be compared with non-corresponding parts of the other; and this is sufficient if the two are similar.

This method was used ; but it is perhaps better to arrange so that each compensator is calibrated independently of the other. In this case alternate readings are taken with and without the cooperation of the additional coils; and the equivalent induction is found for each compensator at various parts of its scale. The following set of readings will give an idea of the *modus operandi.*

Additional coils in.		Additional coils out.	
Reading of I.	Reading of II.	Reading of I.	Reading of II.
9 2°	3 0°	9 2°	42¼°
102½	42¼	102½	53
113	53	113	64
124½	64	124½	75½
135½	75½	135½	86½
148½	86½	148½	98

It will be seen that the adjustment is made alternately on the two compensators. Thus in the second compensator the steps $30°-42\frac{1}{4}°$, $42\frac{1}{4}°-53°$, $53°-64°$, &c. have all the same value, whatever may be the construction of the first compensator, which indeed need not be graduated at all. In like manner, the steps from $92°-102\frac{1}{2}°$, $102\frac{1}{2}°-113°$, &c. on the first compensator have an equal value.

An examination of these and other results, not here recorded, leads to the unexpected conclusion that from 40° to 140°, *i. e.* through a range of 100° about the perpendicular position, the scale of induction does not differ appreciably from the scale of degrees. From 30° to 40°, or from 140° to 150°, the induction is something like a tenth part less than that corresponding to 10° in the neighbourhood of 90°. Within the whole mechanical range of the instruments, from 30° to 150°, there could scarcely be an error of 2 per cent. in assuming M proportional to the angle measured from perpendicularity, *i. e.* $(\frac{1}{2}\pi-\theta)$, or, say, θ'.

The general explanation of this very convenient property is not difficult to understand ; since for high values of θ' the approximation over the whole circumference of coils of not very unequal diameters must lead to a more rapid increase of M than if the smaller coil were very small ; and it is conceivable that for some particular ratio of diameters the increase may just so much exceed that represented by $\sin\theta'$, as to correspond nearly to θ'. I was desirous, however, of explaining this very peculiar relation more completely, and have therefore developed the theory for the case of a ratio of 2 : 1 (nearly that of my apparatus) on the basis of formulæ given by Maxwell. The details of the calculation are given in the form of an appendix (p. 498).

It may suffice here to say that the experimental result is abundantly confirmed ; and that reason is found for the conclusion that the proportionality of induction to angle would be even better maintained if the diameter of the smaller coil were increased from ·50 to ·55 of that of the larger. The non-mathematical reader may be content to accept this proportionality over most of the range of the actual instruments upon the experimental evidence.

The absolute value of the induction-coefficient corresponding to each degree of the compensators was determined at the time of the calibration by comparison with the calculable induction-coefficient between two coils wound in measured grooves cut on the surface of a wooden cylinder. These coils contained respectively 21 and 22 convolutions ; and the induction-coefficient for the mean windings is found to be 277·3 centim. by a calculation of which it is not necessary to record the details. Hence, for the actual coils,

$$M = 21 \times 22 \times 277 \cdot 3 = 1 \cdot 281 \times 10^5 \text{ centim.}$$

The obvious procedure for the comparison would be to combine the compensators without additional coils, so as to obtain a balance at the telephone when both stand at 90°, and then to observe the displacement or displacements necessary when the standard cylinder coils are introduced. In my case, however, the range of induction provided by the compensators was insufficient to balance the standard, if used in this way, even when displacements were made in such (opposite) directions as to cooperate in changing the total induction. An additional pair of coils was therefore introduced, for the purpose, as it were, of shifting the zero, of which nothing required to be known, since they remained connected whether or not the standard coils were in operation. With this modification, balance could be attained in both cases.

With standard coils in, one pair of readings was 130°, $43\frac{1}{2}°$; and with standard coils out, 50°, $127\frac{1}{2}°$. The connections were such that when there was no external change, the corresponding readings would move in the same direction (p. 475); and thus the number of degrees of one compensator equivalent to the standard is

$$130 - 50 + 127\frac{1}{2} - 42\frac{1}{2}, \text{ viz. } 165.$$

Accordingly, every degree of either compensator, not too far removed from the middle of the range, represents 776·3 centim. of induction-coefficient.

The maximum coefficient, when $\theta = 0$, according to Table II. (Appendix), would be about 56100 centim.

In view of the statement * " that the coefficient of mutual induction is less in iron than in copper wires," I may mention an experiment made with the aid of one of the compensators, in which the effect of the substitution of iron for copper is directly examined. The mutual induction measured is that between two circuits, one of which was composed of the two copper coils of 21 and 22 convolutions spoken of above connected in series; and the other of a single turn of wire situated midway between, and lying in a shallow scratch or groove on the wooden cylinder, by which its position was accurately defined. The arrangements being the same as in the determination of the constant of the compensator, the value of the double induction (obtained by reversal) between the circuit of a single turn of copper wire and the circuit of 43 turns was 40°·7. The single turn of copper wire was now replaced by a turn of iron wire of equal diameter and bedded in the same scratch, with the result that the double induction was 40°·6, the same value being obtained whether the iron wire were included in the primary circuit with the battery and interrupter, or in the secondary with the telephone. Care had, of course, to be exercised in the disposition of the leads, in consequence of the use of a single turn only for one of the circuits. So far as the experiment could show, the induction is absolutely the same, whether the single turn be of iron or of copper.

To return now to the bridge arrangement, the following are a few examples of the use of the original form of apparatus. The scale of the wire readings was in $\frac{1}{50}$ inch, the whole length $(Q+R+S)$ being 1960. In ohms the resistance of the whole wire is 4·00. The interrupter was the "reed," making about 1050 (complete) vibrations per second. Thus

$$p = 2\pi \times 1050.$$

The first case is that of a helix of insulated copper wire, without core of any kind. To get a balance the compensator had to be placed at 54°, so that $M = 36°$, each degree representing 776 centim. The resistances also necessary were

$$Q = 610, \quad R = 190 ; \quad \text{therefore} \quad S = 1160.$$

They are expressed in scale-divisions, the value of each of which is

$$\frac{4\cdot00 \times 10^9}{1960} = 2\cdot04 \times 10^6 \frac{\text{centim.}}{\text{sec.}}$$

If, as we are almost entitled to do, we assume that the resist-

ance P to variable currents is the same as that readily found with use of steady currents, viz. 87·3 scale-divisions, we may at once deduce

$$L = M \frac{P+Q+R+S}{S} = \frac{36 \times 776 \times 2833}{1160} = 68200 \text{ centim.}$$

We will, however, dispense with this assumption. Eliminating L between the two equations, we get for the determination of P,

$$P \left\{ 1 + \frac{p^2 M^2}{S^2} \right\} = \frac{Q \cdot R}{S} \left\{ 1 - \frac{p^2 M^2 (Q+R+S)}{S \cdot Q \cdot R} \right\}.$$

In the fractions containing M^2 the resistances must be expressed in absolute measure. We find

$$\frac{p^2 M^2}{S^2} = \frac{4\pi^2 \times 1050^2 \times 36^2 \times 776^2}{1160^2 \times 10^{12} \times 2\cdot 04^2} = \cdot 0061 \;;$$

$$\frac{p^2 M^2 (Q+R+S)}{S \cdot Q \cdot R} = \frac{4\pi^2 \times 1050^2 \times 36^2 \times 776^2 \times 1960}{1160 \times 610 \times 190 \times 10^{12} \times 2\cdot 04^2} = \cdot 1189 \;;$$

so that

$$P = \cdot 876 \frac{Q \cdot R}{S},$$

differing some 12 per cent. from the value (QR/S) given by the usual formula. Inserting the values of Q, R, S, we have

$$P = 87\cdot 5 \text{ scale-divisions.}$$

This is the effective resistance to variable currents of the frequency in question.

With steady currents the readings were

$$Q = 557, \quad R = 190, \quad S = 1213\;;$$

so that

$$P_0 = \frac{557 \times 190}{1213} = 87\cdot 3.$$

The resistance to variable currents, calculated by the correct formulæ with knowledge of the frequency of vibration, is thus almost identical with the value found with steady currents; whereas if we were to ignore the disturbance of the ordinary resistance rule by induction, we should erroneously conclude that the resistance to variable currents was some 12 per cent. higher than to steady currents.

Of other experiments made with this coil I will only mention one. When a stout copper rod was inserted, the circum-

ferential secondary currents induced in it altered the readings
with variable currents to

$$Q = 660, \quad R = 190 ; \quad M = 29\tfrac{1}{2}°.$$

The effective self-induction is evidently diminished, and the
effective resistance increased in accordance with the universal
rule *. The precise values may be obtained from our two
fundamental equations, in the manner exemplified above.

If the foregoing experiment (with the copper core) be
attempted with the scraping-contact interrupter, giving a
mixed sound, no definite balance is obtainable.

The second example that I shall give is of a wire of soft
iron about $1\tfrac{1}{2}$ metre long and $3\cdot3$ millim. diameter. Here
with variable currents from the reed-interrupter, of the same
period as before,

$$Q = 178, \quad R = 190, \quad S = 1592 ; \quad M = 8 \times 776 \text{ centim. ;}$$

from which we find

$$P = \cdot985 \, \frac{Q \cdot R}{S} = 20\cdot93 \text{ scale-divisions.}$$

In the present case the ordinary simple rule (QR/S) would
lead to an error of $1\tfrac{1}{2}$ per cent. only.

The resistance to steady currents is given by

$$P_0 = \frac{100 \times 190}{1670} = 11\cdot38.$$

We may conclude that the effective resistance to variable
currents of this frequency (1050) is $1\cdot84$ times the resistance
to steady currents.

A long length of wire from the same hank was examined
later by another method (p. 488), and gave for the ratio in
question $1\cdot89$.

In some of his experiments † Prof. Hughes found that it
made but little difference to the self-induction of an iron wire,
whether it was arranged as a compact coil of several turns,
or as a single wide loop. The question is readily examined
with the present form of apparatus; for, since the resistance
is not altered, the compensator readings give an accurate
relative measure of the self-induction. A hank of nineteen
convolutions of insulated soft iron wire required for balance
$25°\cdot8$; but when opened out into a single (approximately cir-
cular) loop, the reading was only $11°\cdot2$, a much greater differ-
ence than that mentioned by Hughes.

* See equations (8), (10), (11), (12), Phil. Mag. May 1886, pp. 373–
375.
† Proc. Roy. Soc. vol. xl. p. 457.

A better experiment may be made with a coil of doubled wire. The length previously used was divided into halves, which were tied together closely with cotton thread and bent into a compact coil of 9 convolutions and about $4\frac{1}{2}$ inches diameter. When the two wires were connected in series, in such a manner that the direction of electric circulation was the same in both, the self-induction was represented by $24^{\circ}\!\cdot\!2$; but when one wire was reversed, the self-induction fell to $9^{\circ}\!\cdot\!2$, the large difference depending entirely upon the mutual induction between the two iron wires.

I had intended to apply this apparatus to investigate the self-induction of wires of various materials and diameters, formed into single circular loops, but the subject has been so ably treated by Prof. Weber as to render further work unnecessary, at least as regards the non-magnetic metals.

That the circle is the proper standard form for accurate measurement cannot be doubted; but the effect of magnetic quality is shown most markedly when the wires compared are of given length and diameter, and doubled so as to form single close loops. For a total length $2l$ of copper wire, the self-induction is smallest when the wires are just in contact, and then [*]

$$\mathrm{L} = 3{\cdot}772 \, l.$$

In practice some interval is required for insulation, so that the coefficient of l may perhaps be taken to be 4. To iron wires the theory is not strictly applicable [†], but we may probably assume without serious error

$$\mathrm{L} = l\,(4 \log 2 + \mu) = l\,(2{\cdot}772 + \mu),$$

μ being the *permeability*. Prof. Hughes finds for the ratio of iron to copper under these circumstances $440 : 18$ [‡]; according to which we should have

$$\frac{3 + \mu}{4} = \frac{40}{18},$$

or $\mu = 95$, in approximate agreement with values found by other methods.

Although the original apparatus of Hughes is capable of very good results, and is especially suitable when the wires under test are in but short lengths, the fact that induction and resistance are mixed up in the measurements is a decided drawback, if it be only because the readings require for their interpretation calculations not readily made upon the spot.

[*] Maxwell's 'Electricity and Magnetism,' §§ 686, 688.
[†] Phil. Mag. May 1886, p. 383.
[‡] *Loc. cit.* p. 457.

The more obvious arrangement is one in which both the in-
duction and resistance of the branch containing the subject
under examination are in every case brought up to the given
totals necessary for a balance. To carry this out conveniently
we require to be able to add self-induction without altering
resistance, and resistance without altering self-induction, and
both in a measurable degree. The first demand is easily met.
If we include in the circuit the *two* coils of an induction-com-
pensator, connected in series, the self-induction of the whole
can be varied in a known manner by rotating the smaller
coil. For the self-induction of the instrument, used in this
manner, may be regarded as made up of the constant self-
inductions of the component coils taken separately, and of
twice the positive or negative mutual induction between them.
The first part, in consequence of its constancy, need not be
regarded; and thus every degree (within the admissible
range) may be taken as representing $2 \times 776 \cdot 3$, or $1552 \cdot 6$
cm., of self-induction.

The introduction, or removal, of resistance without altera-
tion of self-induction cannot well be carried out with rigour.
But in most cases the object can be sufficiently attained with
the aid of a resistance-slide of thin German-silver wire. It
may be in the form of a nearly close loop, the parallel out-
going and return parts being separated by a thin lath of wood.
A spring of stout brass wire making contact with both parts
short-circuits a greater or less length of the bight.

In the Wheatstone's quadrilateral, as arranged for these
experiments, the adjacent sides R, S are made of similar
wires of German silver of equal resistance ($\frac{1}{2}$ ohm).
Being doubled they give rise to little induction, but the accu-
racy of the method is independent of this circumstance. The
side P includes the conductor, or combination of conductors,
under examination, an induction-compensator, and the resis-
tance-slide. The other side, Q, must possess resistance and
self-induction greater than any of the conductors to be com-
pared, but need not be susceptible of ready and measurable
variations. But, as a matter of fact, the second induction-
compensator was used in this branch, and gave certain advan-
tages in respect of convenience. Sometimes also a rheostat
was included; but during a set of comparisons the condition
of this branch was usually maintained constant, the necessary
variations being made in P. In order to avoid mutual in-
duction between the branches, P and Q were placed at some
distance away, being connected with the rest of the apparatus
by leads of doubled wire.

It will be evident that when the interrupter acts in the

battery branch, balance can be obtained at the telephone in the bridge only under the conditions that both the aggregate self-induction and resistance in P are equal to the corresponding quantities in Q. Hence when one conductor is substituted for another in P, the alterations demanded at the compensator and in the slide give respectively the changes of self-induction and of resistance.

In this arrangement the induction and resistance are well separated, so that the results can be interpreted without calculation. During the month of July a large number of observations on various combinations of conductors were effected, but the results were not wholly satisfactory. There seemed to be some uncertainty in the determination of resistance, due to the inclusion of the two movable contacts of the resistance-slide in one of the sides (P) of the quadrilateral *. I therefore pass on to describe a slight modification by means of which much sharper measurements were attainable.

In order to get rid of the objectionable movable contacts, some sacrifice of theoretical simplicity seems unavoidable. We can no longer keep Q (and therefore P when a balance is attained) constant ; but by reverting to the arrangement adopted in a well-known form of Wheatstone's bridge, we cause the resistances taken from P to be added to Q, and *vice versâ*. The transferable resistance is that of a straight wire of German silver, with which one telephone terminal makes contact at a point whose position is read off on a divided scale. Any uncertainty in the resistance of *this* contact does not influence the measurements.

Fig. 1.
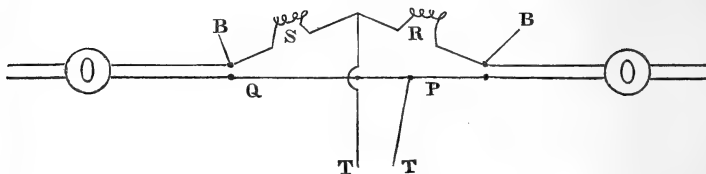
The diagram shows the connection of the parts. One of the telephone terminals goes to the junction of the ($\frac{1}{2}$ ohm) resistances R and S, the other to a point upon the divided wire. The branch P includes one compensator (with coils connected in series), the subject of examination, and part of the divided wire. The branch Q includes the second compensator (replaceable by a simple coil possessing suitable

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* Prof. Hughes appears also to have met with this difficulty in his second apparatus.

self-induction), a rheostat, or any resistance roughly adjustable from time to time, and the remainder of the divided wire. The battery branch, in which may also be included the interrupter, has its terminals connected, one to the junction of P and R, the other to the junction of Q and S. When it is desired to use steady currents, the telephone can of course be replaced by a galvanometer.

In this arrangement, as in the other, balance requires that the branches P and Q be similar in respect both of self-induction and of resistance. The changes in induction due to a shift in the movable contact may usually be disregarded, and thus any alteration in the subject (included in P) is measured by the rotation necessitated at the compensator. As for the resistance, it is evident that (R and S being equal) the value of any additional conductor interposed in P is measured by twice the displacement of the sliding contact necessary to regain the balance.

The position of the contact was read to tenths of an inch ; and, since the actual resistance per inch was ·0246 ohm, a displacement of that amount represents ·0492 ohm. To save unnecessary reductions, the resistance of any conductor will usually be expressed in terms of the contact displacement caused by its introduction, just as the self-induction is expressed in degrees of the compensator.

In order to compare the behaviour of iron and copper, two double coils were prepared as nearly similar as conveniently could be. The iron coil was that already spoken of (p. 479). The resistance of each wire was ·9 ohm, and the diameter ·032 inch. In the double copper coil the resistance of each wire was ·1 ohm, and the diameter ·037 inch. Each coil consisted of 9 (double) convolutions, of diameter about $4\frac{1}{2}$ inches.

The two iron wires being connected in series, the large self-induction (when the current circulated the same way in both wires) was found to be 65°·1 ; the small self-induction (when the directions of circulation were different) was 23°·1. On the other hand, with the copper wires the large self-induction was 45°·0, and the small only 1°·0. Thus, although the manner of connection makes far more *relative* difference in the case of copper than in the case of iron, the absolute difference, which represents four times the mutual induction of the two wires, is nearly the same, viz. 44°·0 for copper and 42°·0 for iron. There is here no evidence of any distinction in the mutual induction of iron and copper, the slight want of agreement being easily attributable to different degrees of

closeness of approach in the two cases. The readings for resistance were sensibly the same, whether the currents were steady (balance being tested by a galvanometer), or were variable with a frequency of 1050 per second. They were also unaffected by the reversal of one of the wires.

With the same pair of double coils an interesting experiment may be made by observing the effect of closing the second wire upon the apparent resistance and self-induction of the first. To steady currents the resistance of one of the copper wires was 1·75, unaltered by closing the circuit of the other wire. With secondary open, the same resistance was found to apply to periodic currents of frequency 1050; but when the secondary was closed the resistance rose to 2·67. On the other hand, the closing of the secondary reduced the self-induction from $11^{\circ}\cdot 2$ to $4^{\circ}\cdot 7$. It will be instructive to compare these results with Maxwell's formulæ :—

$$R' = R + \frac{p^2 M^2 S}{S^2 + p^2 N^2}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

$$L' = L - \frac{p^2 M^2 N}{S^2 + p^2 N^2}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

which we may do by means of a value of M (the mutual induction) deduced from the previous experiment, in which the wires were connected in series. Thus

$$M = 11^{\circ}\cdot 0 = 11\cdot 0 \times 1553 \text{ centim.}$$

From the present experiment,

$$R = S = 1\cdot 75 \text{ inches of slide} = 1\cdot 75 \times \cdot 0492 \times 10^9 \frac{\text{centim.}}{\text{sec.}},$$

$$L = N = 11^{\circ}2 \text{ of compensator} = 11\cdot 2 \times 1553 \text{ centim.,}$$

$$p = 2\pi \times 1050 ;$$

so that

$$\frac{p^2 M^2}{R^2 + p^2 L^2} = \cdot 60.$$

Thus, according to the formulæ, the resistance R′ to the periodic currents should be

$$R' = R + \cdot 60R = 2\cdot 80 \text{ inches of slide.}$$

This compares with the 2·7 actually found. In like manner

$$L' = L - \cdot 60L = 4^{\circ}\cdot 5 \text{ of compensator,}$$

agreeing as well as could be expected with the observed value $4^{\circ}\cdot 7$.

Similar experiments were made on the double coil of iron wire. With secondary open, the resistance of one wire to

steady currents was 16·79. To periodic currents of frequency 1050 the resistance was just perceptibly greater, viz. 16·85, which increased a little further (17·15) when the secondary was closed. The closing of the secondary left the self-induction sensibly unaffected at 21°·2. The much slighter influence of the secondary here observed is due mainly to the higher resistance of the iron as compared with copper. A calculation carried out as before gives

$$\frac{p^2 M^2}{R^2 + p^2 L^2} = ·016,$$

agreeing pretty well with the proportional change observed in the resistance. The corresponding change in self-induction would be barely sensible.

In the case where the primary and secondary circuits are similar (S = R, N = L), Maxwell's general formulæ may be written in the form

$$\frac{R'-R}{R} = \frac{L-L'}{L} = \frac{p^2 M^2}{R^2 + p^2 L^2}; \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

and we may note two extreme cases. When p is small, or, more fully expressed, when the period of vibration is long in comparison with the time-constant of either circuit, viz. L/R, the reaction of the secondary currents is of small importance. On the other hand, when p is large, the right-hand member of (5) approaches to the form M^2/L^2; and this again does not differ much from unity when the two circuits consist of a double coil of non-magnetic wire. Under such circumstances the reaction of the secondary tends to destroy the self-induction and to double the resistance of the primary.

Being desirous of investigating an example approximating to the second extreme, I prepared a double coil of stouter wire than the preceding. The diameter was about ·08 inch, and the length of each wire was 318 inches. There were 20 (double) turns, so that the mean diameter of the coil, wound as compactly as possible, was about 5 inches. The resistance of each wire was about ·05 ohm.

The coefficient of mutual induction was determined by comparison of the self-induction (L) of one wire with that of the two wires connected oppositely in series, viz. (2L − 2M). In this way it appeared that

$$M = 43°·1 = 43·1 \times 1553 \text{ centim.}$$

The interrupter was the reed, of frequency 1050.

Observation showed that the closing of the secondary diminished the self-induction from 44°·4 to 3°·4. The re-

sistance to steady currents was ·92 inch. The resistance to the periodic currents was ·97 with secondary open, and 1·74 with secondary closed.

Taking then

$$L = 44·4 \times 1553 \text{ centim.}, \quad R = ·97 \times ·0492 \times 10^9 \frac{\text{centim.}}{\text{sec.}},$$

we get

$$\frac{p^2 M^2}{R^2 + p^2 L^2} = \frac{10^{17} \times 1·951}{10^{17} \times ·023 + 10^{17} \times 2·071} = ·932.$$

According to the formula, therefore,

$$L' = ·068 \, L = ·068 \times 44·4 = 3°·0,$$

which is to be compared with the observed 3°·4.

The application of the formula to the calculation of R′ is somewhat embarrassed by the observed difference of resistances to steady and to periodic currents when the secondary was open, of which the theory takes no account. It is true that the difference is small, but it appeared to lie outside the limits of error of observation. It is not accounted for merely by the tendency of periodic currents to adhere to the outer parts of a conducting cylinder. If this observation stood alone, one would be inclined to attribute the discrepancy to some action, whether electro-magnetic or electro-static, of the neighbouring secondary, even though open ; but, as we shall have occasion to notice, a similar tendency of the resistance to increase when periodic currents are substituted for steady ones is to be observed in cases where no such explanation is available. The effect was, however, too small to be investigated further without some modification in the apparatus, or in the nature of the conductors submitted to examination.

If we take, as found for the periodic currents, R = ·97, we get

$$R' = 1·93 \times ·97 = 1·87,$$

instead of 1·74 as observed. On the other hand, if we take R = ·92, we get

$$R' = 1·93 \times ·92 = 1·77.$$

The next experiment was contrived to illustrate the behaviour under periodic currents of a system composed of two conductors connected in parallel. The general theoretical formulæ are given in a former paper*; but the more special case selected for experiment was one in which the mutual induction of the two conductors (M) and the self-induction of

* Phil. Mag. May 1886, formulæ (13), (14), p 377.

one of them (N) can be neglected.   The formulæ for the re-
sistance and self-induction of the combination then reduce to

$$R' = \frac{SR}{R+S} \left[1 + \frac{S}{R} \frac{p^2 L^2}{(R+S)^2 + p^2 L^2} \right], \quad \cdot \quad \cdot \quad (6)$$

$$L' = \frac{S^2 L}{(R+S)^2 + p^2 L^2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

in which $SR/(R+S)$ represents the resistance to steady cur-
rents ($p=0$).   The peculiar features of the arrangement are
brought out most strongly by taking a case in which S (the
resistance of the induction-less component) is great compared
with R.   It is then obvious that steady, or slowly alternating,
currents flow mainly through R, and accordingly that the re-
sistance and self-induction of the combination approximate to
R and L respectively.   Rapidly alternating currents, on the
other hand, flow mainly through S, so that the resistance
of the combination approximates to S, and the self-induction
to zero.   These common-sense conclusions are of course
embodied in the formulæ.

The conductors combined in parallel were (1) the coil of
stout copper (p. 483) with its two wires permanently con-
nected in parallel so as to give maximum self-induction (L),
and (2) a moderate length of somewhat fine brass wire.
With steady currents the resistances were

$$R = \cdot 45, \quad S = 2\cdot 29, \quad R_0' = \cdot 35.$$

It had been expected that the resistances R, S, of the sepa-
rate conductors would have been sensibly the same whether
tested by steady or by periodic currents ; but the resistances
in the latter case tended always to appear higher.   Thus with
the same reed as interrupter,

$$R = \cdot 52, \quad S = 2\cdot 33, \quad L = 43^{\circ}\cdot 7, \quad N = \cdot 3^{\circ};$$

and for the combination,

$$R' = 2\cdot 04, \quad L' = 3^{\circ}\cdot 0.$$

These results of observation illustrate satisfactorily the general
behaviour of the combination to periodic currents of high
frequency, and they agree fairly well with the formulæ.
According to these, if we take the values of R and S as ob-
served with periodic currents, we have

$$R' = 2\cdot 16, \quad L' = 2^{\circ}\cdot 61.$$

The altered distribution of current under the influence of
induction, and consequent increase of resistance, exemplified

in the above examples, is an extreme case of what may happen to a sensible extent within a simple conducting cylinder, especially of iron, when the diameter is not very small in relation to the frequency of electrical vibration. In order to avoid magnetizing the material of the conductor, the current tends to confine itself to the outer strata, in violation of the condition of minimum resistance. Prof. Hughes has already given examples of this effect; but they are difficult to compare with theory in consequence of his employment of a vibration of indefinite pitch. The following observations were made with the usual reed interrupter, giving about 1050 vibrations per second.

A somewhat hard Swedish-iron wire, 10·03 metres long, and 1·6 millim. diameter, was first examined. The resistance to steady currents was 10·3, and to the variable currents given by the reed, 12·0. The wire was then softened in the flame of a spirit-lamp, after which the resistance to steady currents was 10·4, and to variable currents 12·1. Expressed in ohms, the resistance to steady currents is

$$10\cdot4 \times \cdot0492 = \cdot51 \text{ ohm.}$$

From these data we may deduce an approximate value of the magnetic permeability (μ) of the material for circumferential magnetization. For if l be the length, R the resistance to steady currents, $p/2\pi$ the frequency of vibration, we have for the resistance (R′) to variable currents the approximate expression *

$$\mathrm{R'} = \mathrm{R}\left\{1 + \frac{1}{12}\frac{p^2 l^2 \mu^2}{\mathrm{R}^2} - \frac{1}{180}\frac{p^4 l^4 \mu^4}{\mathrm{R}^4} + \dots\right\};$$

so that for the rough determination of μ we may take in the present example,

$$\frac{1}{12}\frac{p^2 l^2 \mu^2}{\mathrm{R}^2} = \frac{1\cdot7}{10\cdot4}.$$

The result is $\mu = 108.$

A more accurate use of the formula would bring out a sensibly higher value; but it is hardly worth while to pursue the matter, inasmuch as any deduction of μ from the small observed difference of resistance (1·7) is necessarily subject to considerable error.

In order to get better materials for a determination of μ by this method, a stouter wire of Swedish iron was next tested, 18·34 metres in length and 3·3 millim. in diameter. The metal was rather hard. The resistance to steady currents was found to be 4·7, and to the variable currents from the

* Phil. Mag. May 1886, equation (19) p. 387.

reed 8·9.   These are, as usual, in terms of the scale of the apparatus.   The absolute resistance to steady currents

$$R = \cdot 230 \times 10^9 \; \frac{\text{centim.}}{\text{sec.}}.$$

In this example, the change of resistance (in the ratio 1·89 : 1) is so great that no use can be made of the approximate formula quoted above, but we must revert to the original series.   In the notation employed in the paper referred to, if $\phi(x)$ denote the function *,

$$1 + x + \frac{x^2}{1^2 \cdot 2^2} + \cdots + \frac{x^n}{1^2 \cdot 2^2 \ldots n^2} + \cdots,$$

the resistance to variable currents (R'), and the self-induction, L', are given by

$$\frac{R'}{R} + ip \frac{L'}{R} = \frac{ipl}{R} A + \frac{\phi(ipl\mu/R)}{\phi'(ipl\mu/R)}; \quad \cdots \quad (8)$$

so that the *real* part of the fraction ϕ/ϕ' gives the ratio R'/R. By calculation from the series I find

$$\frac{\phi(i \times 5\cdot 2365)}{\phi'(i \times 5\cdot 2365)} = \frac{-4\cdot 5893 + i \times 1\cdot 5171}{-1\cdot 0297 + i \times 1\cdot 6662} = 1\cdot 8906 + i \times 1\cdot 5859,$$

in which the first term on the right agrees sufficiently nearly with the observed value of R'/R.   We may conclude that

$$pl\mu / R = 5\cdot 2365,$$

whence

$$\mu = 99\cdot 5.$$

In order to give an idea of the degree of accuracy with which μ is determined by the observed value of R'/R, it may be worth while to record another numerical result, viz. :—

$$\frac{\phi(i \times 5\cdot 6815)}{\phi'(i \times 5\cdot 6815)} = 1\cdot 9596 + i \times 1\cdot 6544.$$

In these calculations it is assumed that the increase in R, observed when variable currents are substituted for steady ones, is due simply to a less favourable distribution of current over the section.   If there were sensible hysteresis in the magnetic changes, R would be still further increased.   I believe, however, that under such magnetizing forces as were at play in these experiments, there is no important hysteresis, and that μ may be treated as sensibly constant.

The increased self-induction and resistance of an iron wire,

* The relation of ϕ to Bessel's function of order zero is expressed by
$$\phi(x) = J_0(2i\sqrt{x}).$$

due to its magnetic quality, are doubtless disadvantages from a telephonic point of view. If found serious they may be mitigated, as Prof. Hughes has shown, by the use of a stranded wire, in which the circumferential magnetic circuits are interrupted. There has been some confusion, I think, in connection with the notion of "retardation." If we had the means of observing the passage of signals at various points of a long cable, we should find them not merely retarded (which would be of no consequence) as we recede from the sending end, but also attenuated. The amplitude of a periodic, *e. g.* telephonic, current sent into a cable becomes less and less as the distance increases. Nothing of the kind can happen in a well-insulated iron wire of negligible electrostatic capacity. Its resistance and self-induction may oppose the entrance of a current, but whatever current there is at any moment at the sending end of the wire must exist unimpaired throughout its whole extent.

I will now record a few experiments as to the effect of an iron core upon the apparent self-induction and resistance of an encompassing helix. The wire was wound in one layer upon a glass tube ; the total number of turns is 205, occupying a length of 28·6 centim. The length of the wires forming the cores was 24·1 centim. The results given are the differences of the readings obtained with and without the cores, so that the resistance and self-induction of the helix itself are not included. The interrupter was the same reed as in previous experiments.

A comparison was made of the effect of a solid iron wire 1·2 millim. in diameter and of two bundles of wires of similar iron (drawn from the same specimen) of equal aggregate section and weight. One bundle contained 7 wires, and another 17. The results were :—

	1 wire.	7 wires.	17 wires.
Resistance..............	1·3	0·3	0·2
Self-induction	13°	18°	18°

showing that when the wire was undivided the secondary currents developed in it increased the apparent resistance of the helix by 1·3, and diminished the apparent self-induction.

A similar experiment was tried with a stouter wire, 3·3 millim. in diameter (from the same hank as the length of 18·34 metres treated as a conductor). In the hard condition

the self-induction due to this was $24\frac{1}{2}°$, and the resistance 3·8; numbers altered to $28\frac{1}{2}°$ and 4·4 respectively by softening with a spirit-flame. The effect of a bundle of thirty-five soft wires of the same iron and of equal aggregate section was 84° of self-induction and 1·6 of resistance *.

There is nothing surprising in the conclusion, forced upon us by the observations, that the magnetic effects of iron rods 3·3 millim. in diameter are seriously complicated by the formation of induced internal electric currents. As I have shown on a former occasion †, the principal time-constant of a cylinder of radius a, specific resistance ρ, and permeability μ, is given by

$$\tau = \frac{4\pi\mu a^2}{(2·404)^2\rho}.$$

This means that circumferential currents started and then left to themselves would occupy a time τ in sinking to e^{-1} of their initial magnitude. Whether the effects of such currents will be important or not depends upon the relative magnitudes of τ and of the period of the magnetic changes actually in progress. In the present case, with

$$\mu = 100, \quad \rho = 9827, \quad 2a = ·33,$$

the value of τ is about $\frac{1}{2000}$ of a second, that is, about *half* the period of the actual electrical vibration.

The theory of an infinite conducting cylinder exposed to periodic longitudinal magnetic force ($\mathrm{I}\,e^{ipt}$) has been given by Lamb ‡, who finds for the longitudinal magnetic induction at any distance r from the axis

$$c = \frac{\mathrm{J}_0(kr)}{\mathrm{J}_0(ka)}\,\mu\,\mathrm{I}\,e^{i\,t}, \quad \ldots \ldots \quad (9)$$

where

$$k^2 = -\frac{4\,\pi\mu i p}{\rho}. \quad \ldots \ldots \ldots \quad (10)$$

When the changes are infinitely slow, c reduces to $\mu\,\mathrm{I}\,e^{ipt}$, as should evidently be the case.

A more complete solution was worked out a little later by

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* It may be worth while to remark that in these experiments no approach to a balance could be obtained when a scraping contact interrupter was used. With the reed there was complete silence, or at most a slight perception of the octave. The failure of the scraping contact is due, of course, to the mixed character of the vibration, and to the fact that the adjustments necessary for balance vary rapidly with pitch.

† Brit. Assoc. Report, 1882, p. 446.

‡ Math. Soc. Proc. Jan. 1884, vol. xv. p. 141.

Oberbeck [*], including what is required for our present pur-
pose, viz. the value of $2\pi \int_0^a c\, r\, dr$. In terms of the func-
tion ϕ previously used (p. 489), (8) becomes

$$c = \mu I\, e^{ipt} \frac{\phi(ip\mu \cdot \pi r^2/\rho)}{\phi(ip\mu \cdot \pi a^2/\rho)}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11)$$

whence is readily deduced

$$2\pi \int_0^a c\, r\, dr = \pi a^2 \cdot \mu I\, e^{ipt} \frac{\phi'(ip\mu \cdot \pi a^2/\rho)}{\phi(ip\mu \cdot \pi a^2/\rho)}, \quad \cdot \quad \cdot \quad \cdot \quad (12)$$

where

$$\phi'(x) = 1 + \tfrac{1}{2}x + \tfrac{1}{3}\frac{x^2}{1^2 \cdot 2^2} + \tfrac{1}{4}\frac{x^3}{1^2 \cdot 2^2 \cdot 3^2} + \quad \cdot \quad \cdot \quad \cdot \quad (13)$$

The mathematical analogy between this problem and that
of the variation of a longitudinal electrical current in a
cylindrical conductor has been pointed out by Mr. Heaviside[†],
who has also given the full solution of the latter. Maxwell's
investigation, somewhat further developed in my paper[‡],
relates principally to that aspect of the question with which
experiment is best able to deal, viz. the relation between the
total current at any moment and the corresponding electro-
motive force.

That the argument in ϕ, ϕ' is the same in (12) as in (8)
will be evident, when it is remembered that R in (8) denotes
the resistance of unit length of the cylinder; so that

$$\frac{l}{R} = \frac{\pi a^2}{\rho}.$$

Hence, if we may assume that the material is isotropic, the
same numerical results are applicable to a given wire in both
problems. But from this point the analogy fails us. What
we require here to express is the ratio of the total magnetic
induction to the external magnetizing force, and not the
inverse relation, corresponding in the other problem to the
expression of the electromotive force in terms of the total
current. The experimental results are the reaction of the
core upon the magnetizing circuit, expressed as alterations of
apparent self-induction and resistance. Now if m be the

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[*] Wied. *Ann.* vol. xxi. (1884), p. 672. There seems to be some error
in the way in which the magnetic constant appears in Oberbeck's solution
(47). According to it (as I understand) a copper core would be without
effect.

[†] Phil. Mag. August, 1886, p. 118.

[‡] Phil. Mag. May 1886, p. 386.

number of turns per unit length in the magnetizing helix and C the current (proportional to e^{ipt}), we have

$$\mathrm{I}\, e^{ipt} = 4\pi m \mathrm{C}\,; \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and for the electromotive force (E) due to the change of magnetic induction in the core, reckoned per unit length,

$$\mathrm{E} = m \frac{d}{dt} \left\{ 2\pi \int_0^a c\, r\, dr \right\}$$
$$= 4m^2\pi^2 a^2 \mu \,.\, ip\mathrm{C}\,.\, \phi'/\phi. \quad . \quad . \quad . \quad (15)$$

In order to interpret this, we must separate the real and imaginary parts of ϕ'/ϕ. If we write

$$\frac{\phi'}{\phi} = \mathrm{P} - i\,\mathrm{Q},$$

then the part of E which is in the same phase as $d\mathrm{C}/dt$ is $4m^2\pi^2 a^2 \mu \,.\, ip\mathrm{C}\,.\, \mathrm{P}$; and the part which is in the same phase as C is $4m^2\pi^2 a^2 \mu \,.\, p\mathrm{C}\,.\, \mathrm{Q}$. The first manifests itself as an increase of self-induction, and the second as an increase of resistance. If $\rho = \infty$, $\mathrm{P} = 1$, $\mathrm{Q} = 0$.

What we require to know for our present purpose is the effect of *introducing* the core; and to obtain this we must subtract any part of E which remains when we put $\rho = \infty$, $\mu = 1$. Calling this E_0, we have

$$\mathrm{E}_0 = 4m^2\pi^2 a^2 \,.\, ip\mathrm{C},$$

and

$$\mathrm{E} - \mathrm{E}_0 = 4m^2\pi^2 a^2 \{ip\mathrm{C}(\mu\mathrm{P} - 1) + \mu p\mathrm{C}\,.\, \mathrm{Q}\}.$$

Thus if $\delta\mathrm{L}$, $\delta\mathrm{R}$ be the apparent augmentations of self-induction and resistance in the helix due to the introduction of the core, reckoned per unit length,

$$\left.\begin{array}{l} \delta\mathrm{L} = 4m^2\pi^2 a^2(\mu\mathrm{P} - 1), \\ \delta\mathrm{R} = 4m^2\pi^2 a^2 \mu \,.\, p\mathrm{Q}. \end{array}\right\} \quad . \quad . \quad . \quad . \quad (16)$$

From the calculation already made for the purposes of the other problem, we have

$$\frac{\phi'(i \times 5{\cdot}2365)}{\phi(i \times 5{\cdot}2365)} = (1{\cdot}8906 + i \times 1{\cdot}5859)^{-1},$$
$$= {\cdot}31047 - i \times {\cdot}26044\,;$$

so that for the stout iron wire of $3{\cdot}3$ millim. diameter and $\mu = 99{\cdot}5$,

$$\mathrm{P} = {\cdot}31047, \quad \mathrm{Q} = {\cdot}26044.$$

With these values the effects δL, δR of the core of $3\cdot3$ millim. diameter may be calculated ; but no very good agreement with observation is to be expected, since the conditions of infinite length, isotropy, &c. were but inadequately satisfied. Inserting in (16) $m = 205 / 28\cdot6$, $a = \cdot165$, $\mu = 99\cdot5$, we get

$$\delta L = 1650, \quad \delta R = 10^6 \times \overline{}9\cdot436.$$

These are expressed in absolute measure, and reckoned per unit length of core. To obtain numbers comparable with the experimental readings, we must multiply by $24\cdot1$ (the length of the core), and reduce δL by division by 1553, and δR by division by $10^9 \times \cdot0492$. The result is

$$(\delta L) = 25^\circ\cdot6, \quad (\delta R) = 4\cdot6;$$

which agree moderately well with the observed values, viz.

$$(\delta L) = 24\tfrac{1}{2}^\circ, \quad (\delta R) = 3\cdot8,$$

If the material composing the core were non-conducting, $P = 1$, and

$$\delta L_0 = 4m^2\pi^2a^2(\mu - 1).$$

The ratio of the actual effect to that which would be got from the same aggregate section of a bundle of wires, infinitely thin and insulated from one another, is thus

$$\frac{\delta L}{\delta L_0} = \frac{\mu P -}{\mu - 1},$$

of which the numerical value in the present example is $\cdot303$. The corresponding ratio of observed effects for the solid wire (softened), and for the bundle of 35 wires of the same aggregate section was

$$28\tfrac{1}{2} / 84 = \cdot339.$$

The general result of these experiments is to support the conclusion arrived at by Oberbeck that the action of iron cores, submitting to periodic magnetizing forces of feeble intensity, can be calculated from the usual simple theory, provided we do not leave out of account the induced internal currents which often play a very important part. Oberbeck's observations were made with the electrodynamometer, and with rather low frequencies of vibration—about one tenth of that used in most of the observations here recorded.

We have seen in several examples that the self-induction of a combination of conductors, being a function of the pitch, admits of an indefinite series of values ; and the question suggests itself to which (if any) of these corresponds the

value obtained by galvanometric observation of the transition from a state of things in which all the currents are zero to one in which they have steady values under the action of a constant electromotive force. In the ordinary theory of Maxwell's method for determining self-induction from the throw of the galvanometer-needle in a Wheatstone's bridge (a resistance-balance having been already secured), the conductor under test is supposed to be simple. The general case of an arbitrary combination of conductors can only be treated by a general method. An investigation founded upon the equations of my former paper* shows that the result which would be obtained by Maxwell's method corresponds to the self-induction of the combination for *infinitely slow* vibrations.

We have supposed that the behaviour of the compound conductor is not influenced by electrostatic phenomena ; otherwise the representation of the part of the electromotive force in the same phase as dC/dt as due merely to self-induction would be unnatural. So far as experiment is concerned, we have no means of distinguishing between an effect dependent upon dC/dt and one dependent upon $\int C\,dt$, for the phase of both is the same. We may contrast two extreme cases—(1) a simple conductor with resistance and self-induction, (2) a simple condenser with resisting leads. In the first case the electromotive force at the terminals is written

$$L \cdot ip\,C + R \cdot C\,;$$

in the second

$$-\mu' \cdot ip^{-1}C + R \cdot C,$$

where μ' represents the " stiffness " of the condenser. If we persisted in regarding the imaginary part in the second case as due to (negative) self-induction, we should have to face the fact that the coefficient becomes infinite as p diminishes without limit.

A number of combinations in which the induction of coils is balanced by condensers are considered by Chrystal in his valuable memoir on the differential telephone†.

In a paper‡ already referred to I have shown that when two conductors in parallel exercise a powerful reciprocal

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* Phil. Mag., May 1886, p. 372. The analysis may be simplified by choosing the first type so as to correspond to steady flow. The coefficients b_{12}, $b_{13}\ldots$, as well as the final values of $\dot{\psi}_2$, $\dot{\psi}_3\ldots$ are then zero, and the result may be expressed,

$$\int \Psi_1\,dt = a_{11}\,\dot{\psi}_1 + b_{11}\!\int \psi_1\,dt.$$

† Edinburgh Transactions, 1879.   ‡ Phil. Mag. May 1886, p. 378.

induction, very curious results may follow the application of a periodic electromotive force. I have lately submitted the matter to experimental test, by which theoretical anticipations have been fully confirmed.

The two conductors in parallel were constructed out of the three wires of a heavy and compact triple coil of copper wire* mounted in a mahogany ring, which has been in my possession for many years. Of these wires two are combined in series (with maximum self-induction) to constitute one of the branches in parallel. The other branch is the third wire of the triple coil, so connected that steady currents would circulate the same way round them all. The variable currents were obtained from a battery and scraping-contact apparatus (p. 472), connected directly. Under these conditions, if the intermittence be rapid enough, the currents distribute themselves in the two branches so as nearly to neutralize one another's magnetizing-power ; and this requires that the current in the single wire should be of about twice the magnitude of the current in the double wire, *and in the opposite direction.* If we call these currents 2 and -1, the current in the mains must be $+1$.

As may be seen from formula (13)†, such a state of things leads to a high equivalent resistance for the system ; and the question might be investigated on this basis with the apparatus already described. I preferred, however, to examine directly whether it were true that the current in one of the branches exceeded that in the main ; and this could be readily done by " tapping " with the telephone. For this purpose the two

Fig. 2.
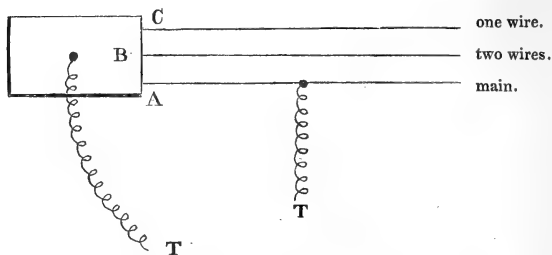
branches and the main were led through short lengths of similar German-silver wire to the junction, composed of a copper plate to which the wires were soldered (fig. 2). One telephone terminal was soldered to the plate ; the other was brought into contact with some point of the German-silver

* The three wires were wound on *together*.   † *Loc. cit.* p. 377

wire carrying the current to be observed.   It is evident that
if the three alternating currents were of equal magnitude,
sounds of like loudness would be heard at equal distances
from the copper plate, whichever of the wires was touched ;
and, further, that the distances required to produce equal
sounds are inversely as the magnitudes of the corresponding
currents.

A moment's observation proved that the currents in A and
B were about equal, and that in C much greater.   Numerical
estimates are best made with the aid of a second observer, who
does not see what contacts are being tried.   My assistant
considered that about $6\frac{1}{2}$ inches of B and about $3\frac{1}{2}$ inches of C
were required to give the same loudness as 6 inches of A.
This agrees with the approximate theory as well as could
be expected.

If the single wire be reversed, then, according to theory
(resistances of German-silver wires neglected), the distribution
should be much the same as of steady currents under the sole
influence of resistance ; that is, the currents in the branches
should be as $+2$ to $+1$, so that on the same scale the main
current would be $+3$.   According to this the equivalent
lengths of the German-silver wires would be 6, 9, 18.   The
numbers actually found by experiment were 6, 8, $17\frac{1}{2}$.

In the first part of this experiment the current in *one* of
the branches is greater than in the main ; but I wished to
examine a case where *both* parts of the divided current
exceeded the whole.   This could be done with a fivefold
coil, as described in the previous paper ; but such was not
ready to hand.   In default thereof a common double coil,
belonging to a large electro-magnet, was enveloped with a
single layer of extra wire, which was combined in series with
one of the original wires.   This arrangement is less favour-
able than one in which the two branches are in close juxta-
position throughout ; but I thought that with the aid of an
iron core it could be made to answer the purpose.   Such a
core was provided in the form of a bundle of fine wires,
solid iron being obviously inappropriate.   The two wires were
now connected in parallel and replaced the triple coil, the
arrangements in other respects remaining unchanged.

The currents in the shorter branch (composed of one
original coil simply), in the longer branch (composed of the
other original and of the additional coil), and in the main
were now found to be inversely as the measured distances
·9, 1·3, 2·3, no regard being paid to sign, viz. as 1·11, ·77,
·43.   These numbers cannot be quite correct as they stand,
for the third should be equal to the difference between the

first and second. If we suppose the second and third to be correct, the first would have to be 1·20 instead of 1·11. Such an error as this may easily occur in estimating the equality of sounds heard successively; and there can be no doubt that the smaller branch current largely exceeded the main current[*].

APPENDIX.—*The Induction-Compensators* (p. 473).

For the mutual induction-coefficient between two circular circuits, subtending angles α_1, α_2 at the point of intersection of their axes (lines through their centres and perpendicular to their planes), and distant c_1, c_2 from that point, Maxwell gives[†]

$$M = 4\pi^2 \sin^2\alpha_1 \sin^2\alpha_2\, c_2 \left\{ \tfrac{1}{2}\frac{c_2}{c_1}\, Q'_1(\alpha_1)Q'_1(\alpha_2)Q_1(\theta) \right.$$
$$\left. + \ \ldots \ + \frac{1}{i(i+1)}\frac{c_2{}^i}{c_1{}^i}\, Q_i{}'(\alpha_1)Q_i{}'(\alpha_2)Q_i(\theta) + \ \ldots \ \right\}, (17)$$

the angle between the axes being denoted by θ. $Q_i \ldots$ denote Legendre's coefficients (more usually represented by P_i), and the *dash* indicates differentiation with respect to μ. In our present application the circuits are *concentric*, so that $\alpha_1 = \alpha_2 = \tfrac{1}{2}\pi$, and c_1, c_2 are equal to their radii. Moreover $\{Q_i{}'(\tfrac{1}{2}\pi)\}^2$ vanishes if i be even; while if i be odd $(2n+1)$ we have

$$\{Q'_{2n+1}(\tfrac{1}{2}\pi)\}^2 = \frac{3^2.\,5^2.\,7^2\,\ldots\,(2n+1)^2}{2^2.\,4^2.\,6^2\,\ldots\,(2n)^2}\ ; \quad . \quad (18)$$

so that

$$M\,\frac{c_1}{4\pi^2 c_2{}^2}\ = \tfrac{1}{2}Q_1(\theta) + \frac{1}{3.4}\,\frac{3^2}{2^2}\left(\frac{c_2}{c_1}\right)^2 Q_3(\theta)$$
$$+ \frac{1}{5.6}\,\frac{3^2.5^2}{2^2.4^2}\left(\frac{c_2}{c_1}\right)^4 Q_5(\mu) + \ \ldots$$
$$+ \frac{1}{(2n+1)(2n+2)}\,\frac{3^2.\,5^2.\,7^2\,\ldots\,(2n+1)^2}{2^2.\,4^2.\,6^2\,\ldots\,(2n)^2}\left(\frac{c_2}{c_1}\right)^{2n}Q_{2n+1}(\theta), (19)$$

which is what we have to calculate for various values of θ on the supposition that $c_2 = \tfrac{1}{2}c_1$.

The following are the values of $Q_{2n+1}(\theta)$ at intervals of $10°$. It is unnecessary for our purpose to go further than Q_7.

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[*] These experiments were described before the British Association at Birmingham, September 3.

[†] 'Electricity and Magnetism,' § 697.

TABLE I.

θ.	$Q_1(\theta)$.	$Q_3(\theta)$.	$Q_5(\theta)$.	$Q_7(\theta)$.
90°	·00000	·00000	·0000	·0000
80	+ ·17365	− ·24738	+ ·2810	− ·2834
70	+ ·34202	− ·41301	+ ·3281	− ·1486
60	+ ·50000	− ·43750	+ ·0898	+ ·2231
50	+ ·64279	− ·30022	− ·2545	+ ·2854
40	+ ·76604	− ·02523	− ·4197	− ·1006
30	+ ·86603	+ ·32476	− ·2233	− ·4102
20	+ ·93969	+ ·66488	+ ·2715	− ·1072
10	+ ·98481	+ ·91057	+ ·7840	+ ·6164
0	+1·00000	+1·00000	+1·0000	+1·0000

From these the values of (19) were computed. They are shown in Table II., together with the sines of θ' and the differences for each step of 10°.

TABLE II. $c_2^2 = ·25\, c_1^2$.

θ.	θ'.	Induction.	Diffs.	Sin θ'.	Diffs.
90°	0°	·0000		·00000	
80	10	·0769	·0769	+ ·17365	·1736
70	20	·1538	·0769	+ ·34202	·1684
60	30	·2304	·0766	+ ·50000	·1580
50	40	·3058	·0754	+ ·64279	·1428
40	50	·3786	·0728	+ ·76604	·1232
30	60	·4460	·0674	+ ·86603	·1000
20	70	·5029	·0569	+ ·93969	·0737
10	80	·5416	·0387	+ ·98481	·0451
0	90	·5559	·0143	+1·00000	·0152

The column headed Induction gives the value of

$$\tfrac{1}{2}Q_1(\theta) + \frac{1}{3.4}\frac{3^2}{2^2}\cdot\frac{1}{4}\,Q_3(\theta) + \frac{1}{5.6}\frac{3^2.5^2}{2^2.4^2}\frac{1}{4^2}\,Q_5(\theta) + \ \cdots$$

It will be seen that for moderate values of θ' the differences are very nearly constant, far more so than the differences of sin θ', which latter would apply to the induction on the supposition of a very small interior coil. The results of the experimental calibration are thus confirmed and explained.

An inspection of the table suggests that the proportionality to θ' might be improved yet further if the value of c_2/c_1 were a little increased. The following numbers calculated for a twenty per cent. increase of c_2^2/c_1^2, viz. for $c_2 = ·54772\, c_1$, confirms the idea. Such a proportion, applicable to the *mean* radii of the coils, might well be designedly chosen.

TABLE III.

$$c_2^2 = \cdot 3 \, c_1^2.$$

θ.	Induction.	Diff.
90°	·0000	
80	·0752	·0752
70	·1508	·0756
60	·2268	·0760
50	·3025	·0757
40	·3769	·0744
30	·4479	·0710
20	·5099	·0620
10	·5532	·0433
0	·5695	·0163

The numbers in the column headed Induction are the values of

$$\tfrac{1}{2}Q_1(\theta) + \frac{1}{3.4}\frac{3^2}{2^2}(\cdot 3)Q_3(\theta) + \frac{1}{5.6}\frac{3^2.5^2}{2^2.4^2}(\cdot 3)^2 Q_3(\theta) + \ \dots$$

The last two entries are liable to a small error from omission of Q_9 &c.

LVIII. *Permanent Magnets.*—III. *On Magnetic Decay (continued).* *By* R. H. M. BOSANQUET, *St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the Philosophical Magazine, ser. 5, xix. 1885, pp. 57–59, I gave an account of the decay of the magnetism of a pair of permanent magnets during some months after they were made.

Observations for H have been continued at intervals with the same pair of magnets, both in the laboratory and the non-magnetic room ; incidentally a large number of determinations of the moments of these magnets have been obtained, and from these an interesting result appears to follow with considerable certainty.

The decay of the magnetism proceeds much more rapidly in summer than in winter; and in all probability the rate of decay is quicker for higher temperatures and slower for lower temperatures.

It will be remembered that Joule proved, by burying systems of magnets for several years, that the maintenance of a constant temperature did not secure the constancy of the magnetic intensity (Reprint, vol. i. page 592), on which Joule

remarks, " so that the cause of the gradual decline of power has yet to be discovered."

Regarding heat as a species of molecular agitation, it seems reasonable to expect that this agitation should act in a manner analogous to mechanical disturbances, in facilitating the decay of permanent magnetism. In fact we know that, by exposing permanent magnets to moderately high temperatures, we can destroy their magnetism altogether.

The following are the mean results of the sets of observations that have been made. The changes in the value of M are shown in the figure. It will be seen that they are much greater in summer than in winter.
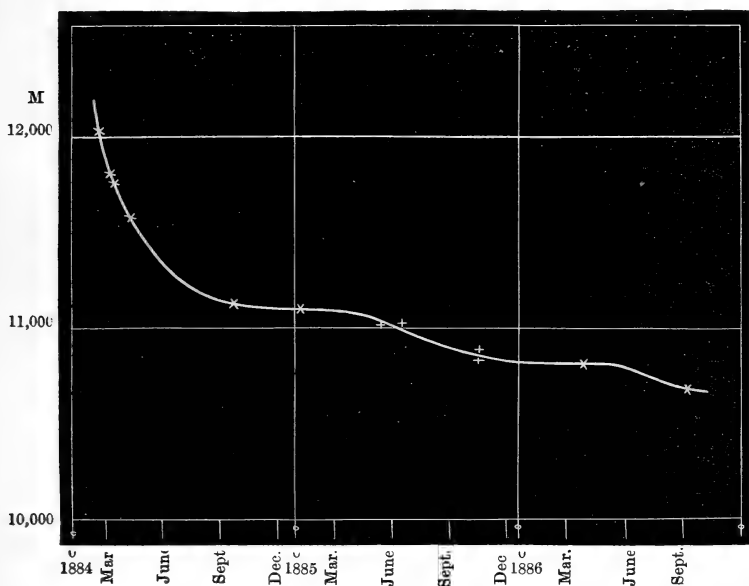
N.B. The temperatures were always noted, and the temperature-coefficients of the magnets were determined by a careful discussion. They are not large enough to produce any difference in the numbers which need be taken into account for the present purpose.

Nonmagnetic Room.

Mean date.	No. of sets.	Temp. F.	M.	H.
1884.				
February 18	9	......	12,039	·18044
March 3	6	......	11,822	·18075
September 18	16	......	11,119	·18020
1885.				
January 4	11	41·8	11,092	·18100
May 28	17	61·4	11,034	·18212
October 24	10	53·5	10,876	·18101
1886.				
April 19	10	50·7	10,826	·18025
September 24	22	60·4	10,656	·18078

Laboratory.

Mean date.	No. of sets.	Temp. F.	M.	H.
1884.				
March 15	12	......	11,767	·17261
April 8	11	......	11,620	·17224
September 18	14	......	11,121	·17370
1885.				
January 2	16	42·7	11,095	·17333
May 6	26	52·6	11,002	·17456
October 24	9	60·1	10,819	·17446
1886.				
April 15	21	47·5	10,805	·17424
September 24	30	58·0	10,661	·17426

LIX. *On the Vapour-pressures of Water from Salt-Solutions.
By* W. W. J. NICOL, *M.A., D.Sc., F.R.S.E., Lecturer on
Chemistry, Mason College, Birmingham*[*].

IN 1835 Legrand[†] published a paper on the effect of various
salts on the boiling-point of water in which they were
dissolved in varying proportions. The results he obtained
were exceedingly interesting, but incomplete; and the experiments of Wüllner[‡] and others have done but little to extend
our knowledge of this branch of the wide subject of solution.
With the desire of arriving at some definite conclusion as

to the nature of the action of salts on the vapour-pressure of the water in which they are dissolved, I commenced in April 1884 a series of experiments on the subject, the first portion of which, relating to the boiling-points of saturated solutions, has been already published*; while the general conclusions arrived at up to the middle of 1885 are to be found in a report on Solution, presented to the British Association in that year. Since then I have repeated my experiments with improved apparatus, with the result that my previous conclusions are extended and confirmed, so that I feel myself justified in publishing this second part of the research.

Before proceeding to an account of my own experiments, it will be advisable briefly to summarize the results obtained by Legrand (*loc. cit.*).

His experiments were conducted as follows:—

After he had determined the boiling-point of pure water by heating it in contact with some granulated zinc in a wide test-tube, in the neck of which was fastened a delicate thermometer the bulb of which was immersed in the water, he introduced a known quantity of the salt under examination and again observed the boiling-point. The tube was weighed, and thus the quantity of water present was ascertained. A further quantity of salt was added, and the operations repeated until a saturated solution was obtained, the amount of salt in which was determined in the ordinary way. From the results thus obtained curves were drawn, and thus the quantities of salt required to raise the boiling-point of water successive half-degrees were found.

On an examination of Legrand's figures, we find that the seventeen salts examined in the above manner are divided into four classes by their effect on the boiling-point of water.

1. Several salts agree in this, that more salt is required to raise the boiling-point the first half-degree than to raise it the second, and this more than the third, and so on : thus, 7·5 per cent. NaCl raise the boiling-point to 101° C. ; 5·7 per cent. raise it from 101° to 102° C.; 4·9 per cent. raise it from 102° to 103° C.; &c. The salts which behave in this way are NaCl, KCl, Na_2CO_3, Na_2HPO_4, and $BaCl_2$.

2. One salt only, $KClO_3$, has its effect on the boiling-point of water represented by a straight line : n (14·64 per cent.) $KClO_3$ raise it $n°$ C.

3. Other salts behave in a manner the converse of those in class 1 ; *i. e.* as the concentration increases so does the amount of salt required to raise the boiling-point each successive degree. NH_4NO_3, $NaNO_3$, and KNO_3 behave in this way.

* Phil. Mag. October 1884.

4. The most general case is a combination of the three foregoing classes in the order given. At first the quantity of salt diminishes rapidly with the concentration; then more slowly till it becomes practically a constant for a few degrees; then it increases more and more rapidly up to the saturation-point. K_2CO_3 may be taken as an instance. From 100°–101° C. 13 per cent. of salt are required; from 101°–102° C. 9·5 per cent. For each degree between 113° and 118° C. 4·8 per cent. only are required; and above this the amount increases till from 130°–131° C. 5·4 per cent are required; and with other salts the increase is still more marked. K_2CO_3, $NaC_2H_3O_2$, $K_2C_4H_4O_6$, NH_4Cl, $CaCl_2$, $SrCl_2$, $Ca(NO_3)_2$, $KC_2H_3O_2$; that is, eight out of seventeen salts examined belong to this class alone.

It appears probable, as indicated above, that there is but a single curve of the form indicated in the diagram below; and that it is complete in the case of some salts, while others correspond to the first, second, or third parts of the curve. How far this hypothesis is borne out by my experiments will be shown later on.
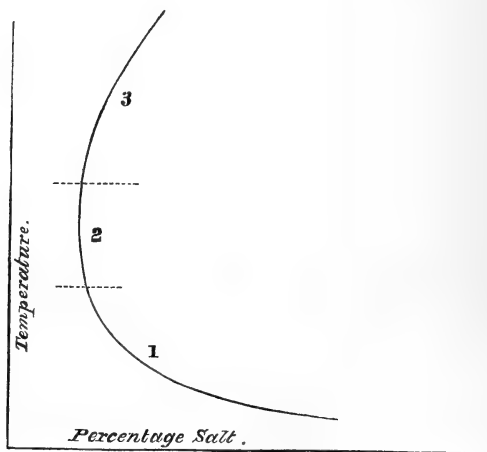
Apart from the general inaccuracy attending Legrand's method of experiment, there is the serious objection to it that, though the pressure was constant, the temperature was variable. It would be difficult, if not impossible, to convert his results into a form in which they would be available for the purposes of the present paper. As it is, I believe they can be used only qualitatively, and that even with hesitation.

In my experiments I had two methods available ; either the barometer-tube method as modified by Konowalow, or the boiling-point method modified so that the temperature was constant and the pressure the variant. The latter is by far the more convenient of the two methods, and is the one that I employed; but it was only after repeated trials that I succeeded in overcoming the danger of superheating and the consequent error of experiment.

A flask, about 200 cubic centim. capacity, communicates by a wide side-tube with an upright condenser ; this in its turn is connected with a three-way tap and an air-reservoir, which communicates with the gauge and the water-pump. The flask stands on a small burner, and is protected from the heat by a metal ring, except about an inch and a half at the very bottom. In the neck of the flask is fitted the thermometer. The course of experiment was as follows :—In the flask was placed 50 grammes of zinc and 60 cubic centim. of water, then $\frac{1}{30}$ of the molecular weight of the salt in grammes were added. This gives a solution of almost exact molecular strength, as $\frac{60}{1800} = \frac{1}{30}$. The thermometer was introduced, care being taken that the bulb was completely immersed ; the side-tube joined to the condenser, the pump set in action, and the burner lighted. When the solution boiled, the pressure was slowly increased till the thermometer rose to 70° C. The barometer and gauge were then read by two telescopes, the pressure was again diminished slightly, a tube full of mercury introduced into the reservoir of the barometer, and another reading made at 70°, the mean of the two being taken. The same operations were repeated at intervals of 5° C. up to 95° C., when the flask was disconnected, washed, and dried, and the whole set of observations repeated with a fresh solution of the same strength. Thus each determination given in the following Tables is the mean of four results, obtained with two separately prepared solutions. The granulated zinc is absolutely necessary in large quantity, otherwise superheating takes place. In no case was the zinc dissolved by the solution ; nor was it appreciably attacked, except by the very strongest solutions of KNO_3 and $NaNO_3$ at the higher temperatures. In these cases appreciable amounts of ammonia were evolved, a point I intend to investigate further ; but even after prolonged boiling the boiling-pressure was found unaltered ; thus the results are not affected by the small decomposition of the salt in these cases. The remainder of the method of experiment is the same as that described in my previous paper. It must not be forgotten that, though the temperatures are arbitrary, still, as the determinations were

all made with the same thermometer and are thus strictly comparable among themselves, and as the boiling-pressures of pure water were determined at the same time with the same thermometer, we have a natural standard by which the results may be corrected if need be.   Nothing, however, would be gained by such a correction ; for, as the figures in Table I. show, the thermometer, which was by Geissler and divided into tenths, was very nearly exact, the results agreeing well with Regnault's, when it is remembered that his figures are for steam and mine for water.   With regard to the probable error for any single determination, that does not much exceed $\pm\,\cdot3$ millim., and is of course proportionately less for the mean results of four determinations which are given in the Tables.

TABLE I.

Boiling-pressures of pure Water (Zinc).

t°.	70°.	75°.	80°.	85°.	90°.	95°.
Regnault ...	233·3	288·8	354·9	433·2	525·5	633·7
Found	228·3	283·2	349·4	428·3	521·4	631·0
Difference ...	5·0	5·6	5·5	4·9	4·1	2·7

At present I have examined only four salts, which may be regarded as typical of the class which crystallize without water ; they are NaCl, KCl, $NaNO_3$, and KNO_3.   I intend soon to examine the behaviour of some typical hydrated salts.

The results thus obtained enable us to examine, not only the effect of varying amounts of salts at the same temperature, but also that of fixed amounts of salts at varying temperatures; that is, the Tables given may be read either vertically or horizontally.   Each Table consists of two halves.   The first contains the value of $p-p'$, where $p =$ the vapour-pressure of pure water at the given temperature as contained in Table I., p' the pressure of water-vapour from the salt-solution of the composition n molecules of salt per 100 molecules of water. The second half of the Table contains $\dfrac{p-p'}{n}$; that is, the effect on the vapour-pressure of pure water by the presence of one salt-molecule in solutions of the stated composition.   A glance at the Tables shows at once that the four salts range themselves in two classes, when the effect of varying amounts of

TABLE II.

n.	70°.	75°.	80°.	85°.	90°.	95°.
$p-p'$ for nNaCl 100 H_2O.						
2.	8·5	10·8	13·7	16·4	20·4	24·6
4.	18·0	22·5	28·0	33·8	40·9	50·2
5.	22·6	28·5	35·2	43·0	52·5	63·3
6.	28·4	35·4	43·5	52·5	63·5	77·0
8.	39·0	48·6	59·9	72·8	88·2	107·1
10.	50·4	62·4	76·6	93·1	112·8	136·4

$$\frac{p-p'}{n} \text{ for } n\text{NaCl 100 } H_2O.$$

n.	70°.	75°.	80°.	85°.	90°.	95°.
2.	4·25	5·40	6·85	8·20	10·20	12·30
4.	4·50	5·63	7·00	8·45	10·23	12·55
5.	4·52	5·70	7·04	8·60	10·50	12·66
6.	4·73	5·90	7·25	8·75	10·58	12·83
8.	4·88	6·08	7·49	9·10	11·03	13·39
10.	5·04	6·24	7·66	9·31	11·28	13·64

salt at the same temperature is considered. In the case of NaCl and KCl, the restraining effect of each molecule of salt increases with the concentration. The reverse holds good

TABLE III.

n.	70°.	75°.	80°.	85°.	90°.	95°.
$p-p'$ for nKCl 100 H_2O.						
2.	7·6	9·6	12·2	14·7	17·4	22·3
4.	16·6	20·5	25·1	31·0	37·4	46·0
6.	25·3	31·2	38·9	47·6	57·8	70·6
8.	34·5	42·7	53·2	64·9	78·7	95·1
10.	44·3	54·7	67·5	82·2	99·7	120·7

$$\frac{p-p'}{n} \text{ for } n\text{KCl 100 } H_2O.$$

n.	70°.	75°.	80°.	85°.	90°.	95°.
2.	3·80	4·80	6·10	7·35	8·70	11·15
4.	4·15	5·13	6·28	7·75	9·35	11·50
6.	4·22	5·20	6·48	7·93	9·63	11·77
8.	4·31	5·34	6·65	8·11	9·84	11·89
10.	4·43	5·47	6·75	8·22	9·97	12·07

TABLE IV.

n.	70°.	75°.	80°.	85°.	90°.	95°.
$p-p'$ for $n\text{NaNO}_3$ 100 H_2O.
2.	8·5	10·9	13·3	16·2	19·7	23·8
4.	16·1	19·6	24·2	29·9	36·0	44·1
5.	19·8	24·6	30·7	37·4	45·6	55·8
6.	22·9	28·6	36·1	44·1	53·9	65·5
8.	30·7	37·9	46·9	57·4	70·0	85·6
10.	36·7	45·8	56·7	69·7	84·9	103·3
15.	51·7	64·2	79·5	97·5	119·3	144·7
20.	65·8	80·9	99·4	122·7	149·4	181·2
25.	76·8	94·8	117·4	144·3	176·2	212·9

$$\frac{p-p'}{n} \text{ for } n\text{NaNO}_3 \text{ } 100 \text{ } H_2O.$$

n.	70°.	75°.	80°.	85°.	90°.	95°.
2.	4·25	5·45	6·65	8·10	9·85	11·90
4.	4·03	4·90	6·05	7·48	9·00	11·03
5.	3·96	4·92	6·14	7·48	9·12	11·16
6.	3·82	4·77	6·02	7·35	8·98	10·92
8.	3·84	4·74	5·86	7·18	8·75	10·70
10.	3·67	4·58	5·67	6·97	8·49	10·33
15.	3·45	4·28	5·30	6·50	7·95	9·65
20.	3·29	4·05	4·97	6·14	7·47	9·06
25.	3·07	3·79	4·69	5·77	7·05	8·52

with NaNO_3 and KNO_3; the more concentrated the solution the less the effect of each molecule of salt present, though the total result increases with the quantity of salt present.

TABLE V.

n.	70°.	75°.	80°.	85°.	90°.	95°.
$p-p'$ for n KNO_3 100 H_2O.
1.	3·9	5·2	6·8	7·8	9·4	11·1
2.	7·5	9·7	12·2	15·0	18·2	21·2
3.	10·0	12·6	15·7	19·7	24·0	29·3
4.	13·4	16·7	20·8	26·0	31·7	39·0
5.	15·8	20·2	25·0	30·8	37·9	46·5
10.	27·3	34·6	43·0	53·7	66·2	81·3
15.	36·5	46·2	57·9	71·8	88·6	108·8
20.	45·0	56·8	71·4	88·4	109·4	134·2
25.	51·2	65·7	82·5	102·0	125·3	153·9

Table V. *(continued).*

$\dfrac{p-p'}{n}$ for n KNO$_3$ 100 H$_2$O.						
n.	70°.	75°.	80°.	85°.	90°.	95°.
1.	3·90	5·20	6·80	7·80	9·40	11·10
2.	3·75	4·85	6·10	7·50	9·10	10·60
3.	3·33	4·20	5·23	6·57	8·00	9·77
4.	3·35	4·18	5·20	6·50	7·93	9·75
5.	3·16	4·04	5·00	6·16	7·58	9·30
10.	2·73	3·46	4·30	5·37	6·62	8·13
15.	2·43	3·08	3·86	4·79	5·91	7·25
20.	2·25	2·84	3·57	4·42	5·47	6·71
25.	2·05	2·63	3·30	4·08	5·01	6·16

The accuracy of the above results is undoubted, though it is at variance with the results obtained by Wüllner (*loc. cit.*), who found that the effect of the above salts was in direct proportion to the quantity present; nor can the conclusions of Wüllner be explained by the fact that his solutions were of percentage composition, while the above are molecular, corresponding to parts per hundred of water ; for though this might explain one class of salts, it fails if applied to the others. My results, on the contrary, are confirmed by those of Tammann [*], obtained by the barometer-tube method, as will be shown later. I reserve the discussion of the probable cause of this different behaviour of the salts to the end of the paper.

The behaviour of the salt-solutions of the same strength, but at different temperatures, has now to be examined, a point which is by no means so simple as the preceding. Perhaps the best method is a comparison of the values of the fraction $\dfrac{p-p'}{np}$ or $1-\dfrac{p'}{np}$, in which the variation in vapour-pressure is eliminated. The values of $\left(\dfrac{p-p'}{np}\times 10,000\right)$ are given in Tables VI., VII., and VIII. for NaCl, NaNO$_3$, and KNO$_3$; that for KCl being practically a constant for each solution and independent of the temperature.

$$
\begin{aligned}
\text{KCl.} \quad n &= 2\left(\dfrac{p-p'}{np}\times 10{,}000\right)=172\pm 5. \\
&= 4 &&=181\pm 2. \\
&= 6 &&=185\pm 2. \\
&= 8 &&=189\pm 2. \\
&=10 &&=193\pm 1.
\end{aligned}
$$

[*] Wiedemann's *Ann.* xxiv.

TABLE VI.

$\dfrac{p-p'}{np} \times 10,000$ for n NaCl 100 H_2O.						
$n.$	70°.	75°.	80°.	85°.	90°.	95°.
2.	186	191	196	191	196	195
4.	197	192	200	197	196	199
5.	198	201	202	201	202	201
6.	207	208	208	204	203	203
8.	214	215	214	214	212	212
10.	221	220	219	217	216	216

TABLE VII.

$\dfrac{p-p'}{np} \times 10,000$ for n NaNO$_3$ 100 H_2O.						
$n.$	70°.	75°.	80°.	85°.	90°.	95°.
2.	186	193	190	189	189	189
4.	176	173	173	175	173	175
5.	174	174	176	175	175	177
6.	167	168	172	172	172	173
8.	168	167	167	168	168	170
10.	161	162	162	163	163	164
15.	151	151	152	152	153	153
20.	144	143	142	143	143	144
25.	135	134	134	135	135	135

TABLE VIII.

$\dfrac{p-p'}{np} \times 10,000$ for n KNO$_3$ 100 H_2O.						
$n.$	70°.	75°.	80°.	85°.	90°.	95°.
1.	171	184	195	182	180	181
2.	164	171	175	173	175	172
3.	146	148	150	153	153	155
4.	147	147	149	152	152	155
5.	138	143	143	144	145	147
10.	120	122	123	125	127	129
15.	107	109	111	112	113	115
20.	99	100	102	103	105	106
25.	90	93	95	95	96	98

Though not very much can be gathered from the individual results, still I think that they give reliable results when treated *en masse.* When we divide the whole of the figures for each

salt into two groups—A, for temperatures 70°, 75°, and 80°; B, for 85°, 90°, and 95°—and add together all the figures in group A and group B, then we find that, in the case of NaCl, group A exceeds group B by 20, with KCl A=B, and with NaNO₃ and KNO₃ group B exceeds group A by 30 and 91 respectively; thus showing that the net result for all strengths of solutions of the salts is that rise of temperature diminishes the restraining effect of the salt in the case of NaCl, does not affect it in the case of KCl, but increases it in the case of NaNO₃ and KNO₃, and more so with the latter than with the former. I am fully aware of the danger of drawing conclusions from differences so small as these; still the number of experiments in each of the groups (eighteen being the minimum) is probably sufficiently great to justify my doing so. In any case, the accuracy of the results is at least as great as those attainable by the barometric method; and I fail to see that more accurate results can be in any way obtained.

Comparing now the weak solutions with the strong ones at the low and high temperatures, we find that, taking the figures in Tables II. to V., and subdividing the groups A and B at the dotted lines, and calling these subgroups A_w and A_s, B_w and B_s respectively, then the ratio of $\dfrac{A_s}{A_w}$ to $\dfrac{B_s}{B_w}$ is:— For NaCl, 1 : 0·983; for KCl, 1 : 0·990; for NaNO₃, 1 : 0·999; for KNO₃, 1 : 1·018; that is (in words), concentration increasing and temperature rising, the result is diminution of the restraining effect of the salt—considerable in the case of NaCl, appreciable with KCl, little or none with NaNO₃, and, on the contrary, a considerable increase in the restraining effect of KNO₃.

Turning now to the results of Tammann (*loc. cit.*), we find general confirmation of the conclusions arrived at above. Tables IX. and X. contain his results compared with mine for the four salts examined by me. Tammann made his determinations at no definite temperatures, but at irregular intervals on the temperature-scale, and he employed the vapour-pressure of water observed at the same time with the salt-solutions as his temperature-indicator ; nor did he use molecular solutions. The quantity of salt present is expressed in parts per 100 of water; and he gives the observed values of T−T′, *i. e.* (*p*−*p*′) and also what he terms " die relative Spann-kraftserniederungen," or relative diminution of vapour-pressure ; this is $\dfrac{(\mathrm{T} - \mathrm{T}') \times 1000}{\mathrm{T} \cdot m}$, where m=parts of salt per

100 water. This is connected with the value $\dfrac{p-p'}{np} \times 10,000$, which I have used above in the following way:—

$$n = \frac{m}{\dfrac{a}{18}}, \text{ where } a = \text{mol. wt. of salt,}$$

$$\therefore \left(\frac{(p-p') \times 1000}{pm} \middle/ \frac{1}{\dfrac{a}{18}} \right) \times 100 = \frac{p-p'}{np} \times 10,000.$$

Now as Tammann's temperatures are variable, it is necessary to use this value in comparing his results; and, again, as his figures vary within rather wide limits, better results are obtained by taking the mean value of $\dfrac{T-T'}{mT} \times 1000$ for temperatures lying on either side of the desired temperature.

Table IX. compares results for $t^\circ = 70^\circ$ and 90°, and for various strengths of NaCl, and for various solutions of the

TABLE IX.

Salt.	TAMMANN.			NICOL.		
	t°.	n.		t°.	n.	
NaCl	71·2	4·55	190	70°	5	198
,, ...	71·2	6·75	208	,,	6	207
,, ...	71·2	10·97	219	,,	10	221
NaCl	90·8	4·55	190	90°	5	202
,, ...	90·8	6·75	201	,,	6	203
,, ...	90·8	10·97	216	,,	10	216
KCl	71·6	3·33	179	70°	4	181±2
,, ...	,,	6·45	183	,,	6	185±2
NaNO₃ ...	72·0	3·22	170	70°	4	176
,, ...	,,	5·58	164	,,	6	167
,, ...	,,	11·12	160	,,	10	161
,, ...	,,	19·30	136	,,	20	144
KNO₃	69·8	2·26	151	70°	3	144
,, ...	72·7	3·15	111	,,	3	146
,, ...	69·8	7·31	95	,,	5	138
,, ...	,,	15·31	96	,,	15	107
,, ...	,,	20·59	96	,,	20	99

other salts at $t^\circ = 70^\circ$. The agreement is, on the whole, satisfactory, and my general conclusions are completely supported.

In Table X. I have taken the whole series of Tammann's results, and divided it in two from the temperature point of view, and then taken the mean value in each half. Thus the values obtained are not quantitatively comparable with mine, but are qualitatively so, inasmuch as they show the effect of

<div align="center">TABLE X.</div>

Salt.	$n.$	t°_{I}	t°_{II}	$t^\circ_{\text{I}} : t^\circ_{\text{II}}$	
NaCl	4·55	196	192	>	SELF.
,, ...	6·75	208	203	>	>20
,, ...	10·97	220	205	>	
KCl	3·33	180	182	< ?	=
,, ...	6·45	181	185	<	
NaNO$_3$ ...	3·22	144	169	<	
,, ...	5·58	152	167	<	<30
,, ...	11·12	152	158	<	
,, ...	19·30	139	142	<	
KNO$_3$......	2·26	150	156	<	
,, ...	3·15	114	119	<	
,, ...	7·31	92	95	<	<91
,, ...	15·31	98	103	<	
,, ...	20·59	98	109	<	

rise of temperature on the vapour-pressure with solutions of constant strength. Here also there is confirmation of my results, and we arrive at the following conclusions :—

1. *Temperature constant.*—The effect of increase of concentration is, in the case of NaCl, an increase of the restraining effect of each salt molecule. Such is also the case, though
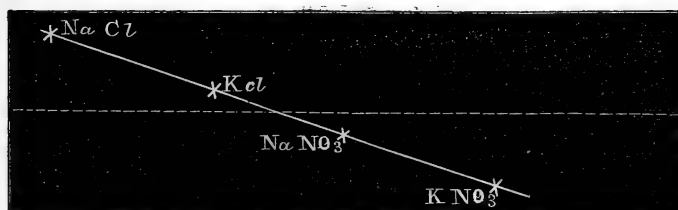
less marked, with KCl, while with NaNO$_3$ and KNO$_3$ precisely the reverse is the case, the four salts forming a series, as is shown diagrammatically above.

2. *Concentration constant.*—The effect of rise of temperature is to diminish the restraining effect of the salt in the case of NaCl, to leave KCl unaffected, and to increase the effect with NaNO₃ and KNO₃. The four salts forming again a series in the same order as above.
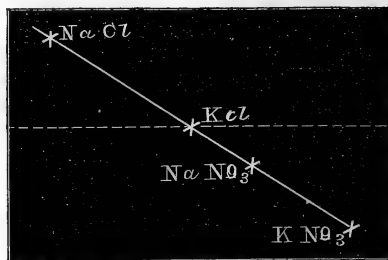
3. When both temperature and concentration increase the salts form the same series—diminution of the restraining effect with NaCl and KCl, little or none with NaNO₃, and a marked increase with KNO₃.

4. When the solubility as a function of the temperature is considered, the same series is observed. The solubility of NaCl increases only slightly with rise of temperature, that of KCl rather more so. Still more marked is the case with NaNO₃, while the solubility of KNO₃ increases enormously with the temperature.

5. There is clearly a connection between increase of solubility of the salts and the effect they have on the vapour-pressure of the water in which they are dissolved.

Any attempt to explain the behaviour of the salts must of necessity, in our present state of knowledge of the nature of solution, partake largely of the nature of hypothesis. Still I cannot refrain from pointing out how completely the Theory of Solution I put forward some years ago explains these very varied phenomena.

According to this theory, solution is an entirely physical process, and results from the action of purely physical forces; thus differing entirely from all the modifications of the Hydrate Theory which have hitherto found favour with experimenters. Solution is the result of the tendency of three forces towards equilibrium. These three forces being the attraction of water for salt, that of salt for salt, and that of water for water. For the sake of brevity the attraction between similar molecules may be spoken of as Cohesion, that between dissimilar molecules as Adhesion. Thus, where the salt is soluble, the adhesion of salt and water is greater than the cohesion of the salt *plus* the cohesion of the water. When the salt is insoluble, the reverse is the case. When the salt dissolves, the adhesion of water to the salt is more and more diminished by the presence of the dissolved salt till a point is

reached at which the three forces are in equilibrium; this is the saturation-point for the temperature under consideration. At the saturation-point as many salt molecules meet and unite to form solid salt in unit time as are dissolved by the action of the water on the still undissolved salt.

The effect of temperature on each of these three forces is to diminish each of them. It may be each to the same extent, or it may be by unequal amounts; and thus a salt may have its solubility unaltered, increased, or diminished by rise of temperature. That is, according as the difference between the adhesion and the sum of the cohesions at the higher temperature is $>$, $=$, $<$ the same difference at the lower temperature, so is the solubility at the higher temperature $>$, $=$, $<$ than the solubility at the lower temperature. But this is not all, for the magnitude of these differences depends on the various effects of temperature on the adhesion and the sum of the cohesions; and this may be very different with different salts, for though the cohesion of the water is affected by temperature always to the same extent, still the cohesion of the salt and the adhesion are variables. With this preliminary explanation, I will proceed at once to the explanation of the effect of salts on the vapour-pressure of water according to the above theory.

The first point to be noted is that all four salts have, in dilute solution, very nearly the same restraining effect :—

n.	NaCl.	KCl.	$NaNO_3$.	KNO_3.
2	4·25	3·80	4·25	3·75
10	5·04	4·43	3·67	2·73

but that in strong solutions it is very different. Now the restraining effect of a salt can only be due to the adhesion of the water and salt[*]; this, then, is very nearly a constant for all four salts, and is still more nearly a constant for salts of the same metal. Next, the heat of solution of the salts is as follows :—

NaCl.	KCl.	$NaNO_3$.	KNO_3.
-1180	-4400	-5200	-8500,

still the same series before observed. Now, as pointed out in another paper, if the restraining effect of salt be a constant, or nearly so, then the heat of solution of various salts is a

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[*] It may be noted, in passing, that unless a salt molecule were capable of disturbing the equilibrium of the *whole* of the water molecules present, it could have no effect on the vapour-pressure of the water.

measure of the physical work done in effecting the change of state of the salt—that is, in overcoming the cohesion of the salt. If this is so, then the cohesion of the four salts under consideration increases from NaCl to KNO_3.

Now the effect of concentration is to markedly increase the restraining effect of the salt in the case of NaCl, to a less extent with KCl ; and, on the contrary, to diminish it with $NaNO_3$, and still more so with KNO_3. But the cohesion is small with NaCl and increases up to KNO_3. Is it not reasonable to suppose that, in the case of a salt with small cohesion, concentration has but little effect on the adhesion ; and that, on the contrary, when the cohesion is very large, the effect of concentration is to slowly diminish the adhesion in each case till the saturation-point is reached ; and salts with cohesion intermediate between the extremes above will have an intermediate effect on the vapour-presure of the water ?

We have next to consider the effect of rise of temperature on the restraining effect of the salt. This we have seen is a diminishing one with NaCl, *nil* with KCl, and an increase in the case of $NaNO_3$ and KNO_3. This, I submit, is fully in agreement with the above, and also with the observed solubilities ; for though an increase of solubility implies an increase of the difference between adhesion and the sum of the cohesions, still that is in few cases due to an actual increase of the value of the adhesion, but to the decrease of the cohesions exceeding the decrease of the adhesion. Now in a solution of a salt of constant strength, rise of temperature is necessarily attended by a decrease in the value of all the three forces. If the cohesion be diminished a little more than the adhesion the salt will be a little more soluble, and the effect on the restraining effect of the salt will be to diminish it ; for though the comparative value of the adhesion be increased, the absolute value is diminished. On the other hand, if the cohesion be very large and be largely affected by rise of temperature, the result will be to increase the comparative value of the adhesion ; and in strong solution to actually increase the absolute value, as is shown by the increase in the restraining effect of KNO_3, especially in strong solutions.

Thus it appears that this theory of solution is able to explain the very varied phenomena of vapour-pressures of water from salt-solutions, as it has explained other phenomena of solution. It remains to be proved whether this explanation is correct.

LX. *On Stationary Waves in Flowing Water.*—Part III.
By Sir William Thomson, *F.R.S.*

[Continued from p. 452.]

In No. 138 (November), p. 446, equation (6), *for* $(gD + ^2)$
read $(gD + q^2)$.

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A S promised in Part I., we may now consider the appli-
cation of the principles developed in it and in Part II.
to the question of towing in a canal, and we shall find almost
surprisingly a theoretical anticipation, $49\frac{1}{2}$ years after date,
of Scott Russell's brilliant " Experimental Researches into the
Laws of certain Hydrodynamical Phenomena that accompany
the Motion of Floating Bodies, and have not previously been
reduced into Conformity with the known Laws of the Resist-
ance of Fluids "*, which had led to the Scottish system of
"fly-boat," carrying passengers on the Glasgow and Ardrossan
Canal, and between Edinburgh and Glasgow on the Forth
and Clyde Canal, at speeds of from 8 to 12 or 13 miles an
hour† by a horse, or a pair of horses, galloping along the
bank.　The practical method originated from the accident of
a spirited horse, whose duty it was to drag a boat along at a
slow speed (I suppose a walking speed), taking fright and
running off, drawing the boat after him, and so discovering
that when the speed exceeded $\sqrt{gD}$ the resistance was less
than at lower speeds.　Mr. Scott Russell's description of the
incident, and of how Mr. Houston took advantage for his
Company of his horse's discovery, is so interesting that I
quote it *in extenso* :—" Canal navigation furnishes at once the
most interesting illustrations of the interference of the wave,
and most important opportunities for the application of its
principles to an improved system of practice.　It is to the
diminished anterior section of displacement, produced by
raising a vessel with a sudden impulse to the summit of the
progressive wave, that a very great improvement recently
introduced into canal transport owes its existence.　As far
as I am able to learn, the isolated fact was discovered acci-
dentally on the Glasgow and Ardrossan Canal of small
dimensions.　A spirited horse in the boat of William Houston

* By John Scott Russell, Esq., M.A., F.R.S.E.　Read before the Royal
Society of Edinburgh on April 3, 1837, and published in the 'Transac-
tions' in 1840.

† One mile an hour is English and American reckoning of velocity,
which, when not at sea, signifies 1·60933 kilometres per hour, or ·44704
metre per second.

Esq., one of the proprietors of the works, took fright and
ran off, dragging the boat with it, and it was then observed,
to Mr. Houston's astonishment, that the foaming stern surge
which used to devastate the banks had ceased, and the
vessel was carried on through water comparatively smooth,
with a resistance very greatly diminished.   Mr. Houston had
the tact to perceive the mercantile value of this fact to the
Canal Company with which he was connected, and devoted
himself to introducing on that canal vessels moving with this
high velocity.   The result of this improvement was so valuable
in a mercantile point of view, as to bring, from the conveyance
of passengers at a high velocity, a large increase of revenue to
the Canal Proprietors.   The passengers and luggage are con-
veyed in light boats, about sixty feet long and six feet wide,
made of thin sheet iron and drawn by a pair of horses.   The
boat starts at a slow velocity behind the wave, and at a given
signal it is by a sudden jerk of the horses drawn up on the top
of the wave, where it moves with diminished resistance, at
the rate of 7, 8, or 9 miles an hour" *.

The "diminished anterior section of displacement pro-
duced by raising a vessel with a sudden impulse to the
summit of the progressive wave" is no doubt a correct
observation of an essential feature of the phenomenon;
but it is the annulment of "the foaming stern surge which
[at the lower speeds] used to devastate the banks" that
gives the direct explanation of the diminished resistance.
It is in fact easy to see that when the motion is steady, no
waves can be left astern of a boat towed through a canal at a
speed greater than $\sqrt{g\mathrm{D}}$, the velocity of an infinitely long
wave in the canal ; and therefore (the water being supposed
inviscid) the resistance to towage must be *nil* when the velo-
city exceeds $\sqrt{g\mathrm{D}}$.   This holds true also obviously for towage
in an infinite expanse of open water of depth D over a plane
bottom.

The formula (25) of Part II. for the whole horizontal com-
ponent force upon an inequality or succession of inequalities
on the bottom allows us to calculate the resistance on a boat
of any dimensions and any shape provided we know the height
of the regular waves which follow it steadily at its own speed
in the canal, at a sufficiently great distance behind it to be
sensibly uniform across the breadth of the canal, according to
the principle explained in the middle of p. 354 of Part I.   The
principles upon which the values of $\mathfrak{H}$ [the h of formula (25),
Part II.] may be calculated are partly given in the remainder

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* Trans. Roy. Soc. Edinb. vol. xiv. (1840) p. 79.

of the present article, and will be more fully developed in Part IV.

To find the steady motion of water flowing in a rectangular channel over a bottom with geometrically specified inequalities, it is convenient, after the manner of Fourier, to first solve the problem for the case in which the profile of the bottom is a curve of sines deviating infinitesimally from a horizontal plane.

For convenience, take OX along the mean level of the bottom, positive in the direction of U the mean velocity of the stream; and OY vertical, positive upwards. Let

$$h = \mathrm{H} \cos mx \quad . \quad . \quad . \quad . \quad . \quad (1)$$

be the equation of the bottom ; and

$$y - \mathrm{D} = \mathfrak{h} = \mathfrak{H} \cos mx \quad . \quad . \quad . \quad . \quad (2)$$

be the equation of the free surface, $\mathfrak{h}$ being height above its mean level. Let ϕ be the velocity potential; u, v the velocity components; and p the pressure at any point (x, y) of the water at time t : so that we have

$$u = \frac{d\phi}{dx} \text{ and } v = \frac{d\phi}{dy} \quad . \quad . \quad . \quad . \quad (3),$$

and

$$p = \mathrm{C} - gy - \tfrac{1}{2}(u^2 + v^2) \quad . \quad . \quad . \quad . \quad (4).$$

Now the deviation from uniform horizontal velocity is infinitesimal, and therefore v and $u - \mathrm{U}$, are infinitely small. Hence (4) gives

$$p = \mathrm{C} - gy - \tfrac{1}{2}\mathrm{U}^2 + \mathrm{U}(u - \mathrm{U}) \quad . \quad . \quad . \quad (5).$$

ϕ must be a solution of the equation of continuity $\dfrac{d^2\phi}{dx^2} + \dfrac{d^2\phi}{dy^2} = 0$, and the proper one for our present case clearly is

$$\phi = \mathrm{U}x + \sin mx(\mathrm{K}\epsilon^{my} + \mathrm{K}'\epsilon^{-my}) \quad . \quad . \quad . \quad (6),$$

where, because the motion is steady, K and K' are constants. This, in virtue of (3), gives

$$u - \mathrm{U} = m \cos mx \, (\mathrm{K}\epsilon^{my} + \mathrm{K}'\epsilon^{-my}), \quad . \quad . \quad (7) ;$$

$$v = m \sin mx(\mathrm{K}\epsilon^{my} - \mathrm{K}'\epsilon^{-my}) \quad . \quad . \quad . \quad (8).$$

Hence, as the values of y at the bottom and at the surface are infinitely nearly 0 and D respectively, we find respectively for the vertical component velocity at the bottom and at the surface,

$$m \sin mx \, (\mathrm{K} - \mathrm{K}'), \text{ and } m \sin mx \, (\mathrm{K}\epsilon^{m\mathrm{D}} - \mathrm{K}'\epsilon^{-m\mathrm{D}}).$$

Hence, to make the bottom-stream-lines and surface-stream-

lines agree respectively with the assumed forms (1) and (2),
we clearly have

$$m(K - K') = m\mathrm{HU} \quad . \quad . \quad . \quad . \quad . \quad (9),$$

and

$$m(K\epsilon^{mD} - K'\epsilon^{-mD}) = m.\mathfrak{H}U \quad . \quad . \quad . \quad . \quad (10);$$

whence

$$\left. \begin{array}{l} K = U\ \dfrac{\mathfrak{H} - H\epsilon^{-mD}}{\epsilon^{mD} - \epsilon^{-mD}}, \\[3mm] K' = U\ \dfrac{\mathfrak{H} - H\epsilon^{mD}}{\epsilon^{mD} - \epsilon^{-mD}} \end{array} \right\} \quad . \quad . \quad . \quad (11).$$

Now at the free surface the pressure is constant, and hence,
by (5), we have

$$-gy + U(u - U) = \text{constant} \quad . \quad . \quad . \quad (12):$$

from which, by (2), (7), and (11), we find

$$0 = -g.\mathfrak{H} + mU^2\ \frac{\mathfrak{H}(\epsilon^{mD} + \epsilon^{-mD}) - 2H}{\epsilon^{mD} - \epsilon^{-mD}},$$

whence

$$\mathfrak{H} = \frac{2H}{\epsilon^{mD} + \epsilon^{-mD} - \dfrac{g}{mU^2}\left(\epsilon^{mD} - \epsilon^{-mD}\right)} \quad . \quad . \quad (13),$$

which is the solution of our problem, for the case of the
bottom a simple harmonic curve.

Suppose now the equation of the bottom to be

$$h = (\kappa \cos mx + \kappa^2 \cos 2mx + \kappa^3 \cos 3mx + \&c.\,)mA/\pi \quad . \quad (14);$$

the equation of the surface, found by superposition of solu-
tions given by (13), allowable because the motion deviates
infinitely little from horizontal uniform motion throughout
the water, is

$$y - D = \mathfrak{h} = \sum_{i=1}^{i=\infty} \frac{2\kappa^i \cos imx\ .\ mA/\pi}{\epsilon^{imD} + \epsilon^{-imD} - \dfrac{g}{imU^2}\left(\epsilon^{imD} - \epsilon^{-imD}\right)} \quad (15).$$

To interpret the equation (14) by which the bottom is
defined, remark that, by the well-known summation of its
second member, it is equivalent to

$$h = \frac{\frac{1}{2}mA/\pi\ .\ (1 - \kappa^2)}{1 - 2\kappa \cos mx + \kappa^2} - \frac{1}{2}mA/\pi = \frac{mA/\pi\ .\ \kappa\,(\cos mx - \kappa)}{1 - 2\kappa \cos mx + \kappa^2} \quad (16).$$

The series (14) is convergent for all values of κ less than
unity. According to the method of Fourier, Cauchy, and
Poisson, the extreme case of κ infinitely little less than unity
will be made the foundation of our practical solutions. By

(14) we see that $\displaystyle\int_{-\pi/m}^{\pi/m} d\,x\,h = 0$ . . . . . . (17);

and hence by the first of equations (16) we see that

$$\int_{-\pi/m}^{\pi/m} dx\ \frac{\frac{1}{2}\,m\,\mathrm{A}/\pi\,.\,(1-\kappa^2)}{1-2\kappa\cos mx + \kappa^2} = \mathrm{A} \ . \quad . \quad . \quad (18).$$

Now when κ is infinitely little short of unity the factor of dx in the first member of (18) is zero for all values of x differing finitely from zero or $2i\pi/m$, (i being an integer); and it is infinitely great when $x=0$ or $2i\pi/m$. Hence we infer from (17) and (18) that a vertical longitudinal section of the bottom presents a regular row of symmetrical elevations and depressions above and below its mean level; the elevations being confined to very small spaces on the two sides of each of the points $x=0$ and $x=2i\pi/m$, and the profile-area of each elevation being A. The depths of the depressions below the average level in the intermediate spaces between the elevations, are of course extremely small because of the exceeding shortness of the spaces over which are the elevations. For our complete analytical solution, not only must A be infinitely small, but the steepness of the slope up to the summit of A must everywhere be an infinitely small fraction of a radian ; and of course therefore the infinitesimal lowering of the bottom between the ridges, which the adoption of a mean bottom-level for our datum line has necessarily introduced, may be left out of account in our dynamical problem.

If the slope of the ridge is not an infinitely small fraction of a radian our solution will still hold, provided its height is very small in comparison with the depth of the water over it. But the effective potency of the ridge would then not be its profile-area A, but something much greater; of which the amount would be found by taking a stream-line over it, far enough above it to have nowhere more than an infinitesimal slope, and finding the profile-area of such a stream-line above its own average level considered as the virtual bottom. With these explanations we shall speak of a ridge for brevity instead of an " irregularity " or " obstacle," and call its profile-area A, simply the " magnitude of the ridge ;" this being, as we see by (15), the measure of its potency in disturbing the surface. When instead of a ridge we have a hollow, A is negative ; and when convenient we may, of course, call a hollow a negative ridge.

It is clear that (15) converges, and does not depend for its convergence on κ being less than unity ; so that in it we may take κ absolutely equal to unity, and we shall do so accordingly.

To find now the effect of a single ridge, remark that if l be the length from ridge to ridge,

$$m = 2\pi/l \quad . \quad . \quad . \quad . \quad . \quad . \quad (19).$$

After the manner of Fourier now suppose l infinitely large; which makes m infinitely small; and put

$$im = q \text{ and } m = dq \quad . \quad . \quad . \quad . \quad (20);$$

then with $\kappa = 1$, (15) becomes

$$\mathfrak{h} = \int_0^\infty dq \, \frac{2\mathrm{A}/\pi \cdot \cos qx}{\epsilon^{q\mathrm{D}} + \epsilon^{-q\mathrm{D}} - \dfrac{1}{qb}(\epsilon^{q\mathrm{D}} - \epsilon^{-q\mathrm{D}})} \quad . \quad . \quad (21);$$

where

$$b = \mathrm{U}^2/g \quad . \quad . \quad . \quad . \quad . \quad . \quad (22).$$

Equation (21) will be shortened, and for some interpretations simplified, by making $q\mathrm{D} = \sigma$, when it becomes

$$\mathfrak{h} = \int_0^\infty d\sigma \frac{2\mathrm{A}/\mathrm{D}\pi \cdot \cos(\sigma x/\mathrm{D})}{\epsilon^\sigma + \epsilon^{-\sigma} - \dfrac{\mathrm{D}}{b\sigma}(\epsilon^\sigma - \epsilon^{-\sigma})} \quad . \quad . \quad (23).$$

The definite integral (21) or (23) seemed rather intractable, and the quadratures required to evaluate it, for many and widespread enough values of x to show the shape of the surface for any one particular value of D/b, would be very laborious. But I had found a method of evaluating it from the periodic solution for an endless succession of equidistant equal ridges (15), wholly analogous to analytical deductions from corresponding solutions for cases of thermal conduction and of signalling through submarine cables, to be found in vol. ii. pp. 49 and 56 of my Collected Mathematical and Physical Papers; and, towards applying this method to a particular case of the disturbance due to a single ridge, I had fully worked out the periodic solution for the case represented by the diagram of curves (fig. 3, p. 529), when I found a direct and complete analytical solution for the single-ridge problem in a form exceedingly convenient for arithmetical computation, except for the case of x equal to zero, or from zero to a quarter or a half of the depth. The previous method happily gives the solution for small values of x, and indeed for values up to two or three times the depth, by very rapidly converging series, and thus between the two methods we have a remarkably satisfactory solution of the whole problem.

Before explaining the curves and their relation to the problem of the single ridge, I shall give the new direct solution

of this problem. It is founded on a well-known analytical method of Cauchy's, of which examples are given in the Eighteenth note (p. 284) to his Memoir on the Theory of Waves[*].

First, bring the denominator of (23) to the form of the product of an infinite number of quadratic factors, as follows:—
Let

$$W = \frac{1}{2\left(1 - \dfrac{D}{b}\right)} \left\{ \epsilon^{\sigma} + \epsilon^{-\sigma} - \frac{D}{b\sigma}(\epsilon^{\sigma} - \epsilon^{-\sigma}) \right\} \quad . \quad (24).$$

Expanding in powers of σ, we have

$$W = 1 + \frac{1}{1 - \dfrac{D}{b}} \left\{ \frac{1}{1 \cdot 2}\left(1 - \frac{1}{3}\frac{D}{b}\right)\sigma^2 \right.$$
$$\left. + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4}\left(1 - \frac{1}{5}\frac{D}{b}\right)\sigma^4 + \&c. \right\} \quad . \quad (25).$$

Hence, when b is greater than D, W is positive for all real values of σ. But when b has any positive value less than D, W (which is always positive for small values of σ^2) is negative for large values of σ^2; and therefore at least one positive value of σ^2 makes W zero. We shall see presently that only one positive value of σ^2 does so. We shall see that all the zeros of W when b is less than D, and all but one when b is greater than D, correspond to real negative values of σ^2. This indeed is obvious if for σ^2 we put $-\theta^2$, which gives

$$W = \frac{2}{1 - \dfrac{D}{b}} \left(\cos\theta - \frac{D}{b}\frac{\sin\theta}{\theta} \right) \quad . \quad . \quad (26);$$

and which shows that the zeros of W are given by the roots of the well-known transcendental equation

$$\frac{\tan\theta}{\theta} = \frac{b}{D} \quad . \quad . \quad . \quad . \quad . \quad (27).$$

When b is greater than D this equation has all its roots real, and in the first, third, fifth, &c. quadrants. When b is less than D the root in the first quadrant is lost, and in its stead we clearly have a pure imaginary; while the roots in the third, fifth, &c. quadrants remain real. Let θ_1, θ_2, θ_3, &c. be the roots of the first, third, fifth, &c. quadrants. As the first term of equation (25) is unity, we have

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[*] *Mémoires de l'Académie Royale de l'Institut de France, savans étrangers*, tome i. (1827).

$$W = \left(1 - \frac{\theta^2}{\theta_1{}^2}\right)\left(1 - \frac{\theta^2}{\theta_2{}^2}\right)\left(1 - \frac{\theta^2}{\theta_3{}^2}\right) \&c.$$

or

$$W = \left(1 + \frac{\sigma^2}{\theta_1{}^2}\right)\left(1 + \frac{\sigma^2}{\theta_2{}^2}\right)\left(1 + \frac{\sigma^2}{\theta_3{}^2}\right) \&c.$$

$\qquad$. (28) ;

where $\theta_2{}^2$, $\theta_3{}^2$, &c. are real positive numerics, while $\theta_1{}^2$ is real positive or real negative according as b is greater than D or less than D.

Resolving now the reciprocal of W into partial fractions, we find

$$\frac{1}{W} = \frac{N_1}{1 + \dfrac{\sigma^2}{\theta_1{}^2}} + \frac{N_2}{1 + \dfrac{\sigma^2}{\theta_2{}^2}} + \frac{N_3}{1 + \dfrac{\sigma^2}{\theta_3{}^2}} + \&c. \quad . \quad . \quad (29);$$

where

$$N_i = \frac{-1}{\theta_i{}^2\left[\dfrac{dW}{d(\theta^2)}\right]_i} = \frac{-2}{\theta_i\left(\dfrac{dW}{d\theta}\right)_i} = \frac{(1 - D/b)\cos\theta_i}{D/b - \cos^2\theta_i}$$

$$= \frac{(1 - D/b)\sin\theta_i}{\theta_i(1 - b/D \,.\, \cos^2\theta_i)} \quad . \quad (30).$$

For $i = 1$ and $D > b$, θ_i is, as we have seen, imaginary (its square real negative), and for this case the formula (30) may be conveniently written

$$N_1 = -\frac{\frac{1}{2}(D/b - 1)(\epsilon^{\sigma_1} + \epsilon^{-\sigma_1})}{D/b - \frac{1}{2} - \frac{1}{4}(\epsilon^{2\sigma_1} + \epsilon^{-2\sigma_1})} \quad . \quad . \quad . \quad (31);$$

and the equation for finding σ_1 is

$$\epsilon^{\sigma_1} + \epsilon^{-\sigma_1} - \frac{D}{b\sigma_1}(\epsilon^{\sigma_1} - \epsilon^{-\sigma_1}) = 0 \quad . \quad . \quad . \quad . \quad (32),$$

an equation which has one, and only one, real root when $D > b$, and no real root when $D < b$.

When b/D is given, it is easy to find, as the case may be, σ_1 of (32) or θ_1 the first-quadrant root of (27), by arithmetical trial and error; and the successive roots θ_2, θ_3, &c. more and more easily, by the solution of (27). It is to be remarked that, whatever be the value of b/D, these roots approach more and more nearly to the superior limits of the quadrants in which they lie : thus if we put

$$\theta_i = (i - \tfrac{1}{2})\pi - \alpha_i \quad . \quad . \quad . \quad . \quad (33),$$

we have

$$N_i\theta_i = (-1)^{i+1}\frac{(1 - D/b)\sin\alpha_i}{D/b - \sin^2\alpha_i}\left[(i - \tfrac{1}{2})\pi - \alpha\right]$$

$$= (-1)^{i+1}\frac{(1 - D/b)\cos\alpha_i}{1 - b/D\sin^2\alpha_i} \quad . \quad . \quad . \quad . \quad (34);$$

and

$$\sin \alpha_i [(i-\tfrac{1}{2})\pi - \alpha_i] = D/b . \cos \alpha_i \quad . \quad . \quad . \quad (35);$$

or, as is convenient for approximation, when i is very large,

$$\alpha_i [(i-\tfrac{1}{2})\pi - \alpha_i] = D/b . \frac{\alpha_i}{\tan \alpha_i} \quad . \quad . \quad . \quad (36),$$

which shows that as i increased to infinity, the value of α_i approaches asymptotically to $D/b (i-\tfrac{1}{2})\pi$. Hence when i is very large, the second member of (36) becomes approximately $D/b . (1 - \tfrac{1}{3} \alpha_i^2)$; and the equation becomes

$$(1-\tfrac{1}{3}D/b)\alpha_i^2 - (i-\tfrac{1}{2})\pi \alpha_i \doteq -D/b \quad . \quad . \quad (37);$$

a quadratic, of which the smaller root when D is less than $3b$, and the positive root when D is greater than $3b$, is the required value of α_i.

Going back now to (23) and modifying it by (24) and (29), we have

$$\mathfrak{h} = \frac{A/D\pi}{1-D/b} . \Sigma N_i \int_0^\infty d\sigma \frac{\cos \dfrac{x\sigma}{D}}{1 + \dfrac{\sigma^2}{\theta_i^2}} \quad . \quad . \quad . \quad (38);$$

or, according to the well-known evaluation (attributed by Cauchy to Laplace) of the definite integral indicated,

$$\mathfrak{h} = \frac{\tfrac{1}{2}A/D}{1-D/b} . \Sigma \theta_i N_i \epsilon^{-\frac{\theta_i x}{D}} . \quad . \quad . \quad . \quad (39);$$

or with θ_i, N_i eliminated by (33) and (34),

$$\mathfrak{h} = \tfrac{1}{2}A/D . \Sigma \frac{(-1)^{i+1} \cos \alpha_i}{(1-b/D . \sin^2 \alpha_i)} \epsilon^{-\frac{[(i-\frac{1}{2})\pi - \alpha_i]x}{D}} \quad . \quad . \quad (40),$$

where $\alpha_1, \alpha_2, \ldots \alpha_i$ denote all the positive roots of (35).

This series converges with exceeding rapidity when x is any thing greater than D, and with very convenient rapidity for calculation when x is even as small as a tenth of D. When $x=0$, the convergence has the same order as that of $1-e+e^2-$&c., when $e=1$; and we find the sum by taking as remainder half the term after the last term included. The true value of the sum is intermediate between the values which we obtain by this rule for a certain number of terms, and then for one term more. When it is desired to obtain the result with considerable accuracy, a large number of terms would be required; and it will no doubt be preferable to use my first method as indicated above.

It remains to deal with the first term for the case $D > b$, which makes it imaginary in the form (39), but real in the form (38) with $-\sigma_1^2$ substituted for θ_1^2. For this case we

have, by the well-known definite integral, first, I believe, evaluated by Cauchy,

$$\mathfrak{h}_1 = \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sigma_1 N_1 \sin \frac{\sigma_1 x}{D} \quad \cdot \quad \cdot \quad \cdot \quad (41) \; ;$$

where σ_1' and N_1 are given by (32) and (31).

It is to be remarked that, inasmuch as (38) has the same value for equal positive and negative values of x, the evaluations expressed in (39) and (41) are essentially discontinuous at $x = 0$; and when x is negative, $-x$ must be substituted for x in the second member of the formulas. I hope in Part IV. to give numerical illustrations ; but with or without numerical illustrations, the analytical formula (39), with (41) for its first term and the sign of x changed throughout when x is negative, is particularly interesting as a discontinuous expression for a curve passing continuously from one to the other of the two curves

$$y = \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sigma_1 N_1 \sin \frac{\sigma_1 x}{D} \text{ for large positive values of } x$$
and
$$y = -\frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sigma_1 N_1 \sin \frac{\sigma_1 x}{D} \text{ for large negative values of } x$$
$$\left.\right\} (42).$$

For the case of $b > D$ every term of (39) is real, and (remembering that the sign of x is changed when x is negative) we see that it makes $\mathfrak{h}$ equal for equal positive and negative values of x, and diminish asymptotically to zero as x becomes greater and greater in either direction. It expresses unambiguously the solution (clearly unique when $b > D$) of the problem of steady motion of water in a uniform rectangular canal interrupted only by a single ridge of magnitude A across the bottom. This is the case of velocity of flow greater than that acquired by a body in falling through a height equal to half the depth.

It is otherwise in respect to uniqueness of the solution when the velocity of flow is less than that acquired by a body in falling through a height equal to half the depth ($b < D$). For this case the formulas (39) and (41) express a particular solution of the problem of steady motion through a rectangular canal, when regularity of the canal is only interrupted by the single ridge of magnitude A. But we clearly have an infinite number of solutions of this problem ; because in still water in a canal of depth D we can have free waves of any velocity from zero to $\sqrt{gD}$, which is the velocity of an infinitely long wave in water of depth D. In our flowing water then superimpose upon the solution (39) (41), any

wave-motion of arbitrary magnitude, and arbitrarily chosen position for one of the zeros, with wave-length such that the velocity of wave-propagation is U, and the direction of motion such as to cause the progression of the wave to be up-stream. The wave-motion thus instituted constitutes a set of free stationary waves, and the superposition of this upon the case of motion represented by our symmetrical solution constitutes the general solution of the problem of single-ridge steady motion. To find the arbitrary addition which we must thus make to our symmetrical solution to find the general solution, put (13) into the following form :

$$\frac{2H}{\mathfrak{H}} = \epsilon^{mD} + \epsilon^{-mD} - \frac{g}{mU^2}(\epsilon^{mD} - \epsilon^{-mD}) \quad . \quad (43).$$

This shows that if H=0, $\mathfrak{H}$ may have any value (that is to say, we may have stationary waves of any magnitude over a plane bottom) if

$$\epsilon^{mD} + \epsilon^{-mD} - \frac{g}{mU^2}(\epsilon^{mD} - \epsilon^{-mD}) = 0 \quad . \quad (44).$$

This is in fact the well-known equation to find the velocity U relatively to the water, of periodic waves of wave-length $2\pi/m$ in a canal of depth D. For us at present equation (44) is to be looked upon as a transcendental equation for determining the wave-length corresponding to U a given velocity of progress ; and it has, as we have seen, only one real root when $U < \sqrt{gD}$; but no real root when $U > \sqrt{gD}$. Putting now in (43) $U^2 = gb$, and comparing with (32), we see that $mD = \sigma_1$; and going back to equation (2) above we see that

$$\mathfrak{H} \cos \frac{\sigma_1(x-a)}{D} \quad . \quad . \quad . \quad . \quad . \quad (45) ;$$

where $\mathfrak{H}$ and a are arbitrary constants, is the addition which we must make to (39) to give the general solution for the case $b < D$. Putting together this and (39) and (40), we accordingly have for the general solution of the single-ridge steady-motion problem, for the case of $U < \sqrt{gD}$,

$$\mathfrak{h} = C \cos \frac{\sigma_1 x}{D} + (C' + \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sigma_1 N_1) \sin \frac{\sigma_1 x}{D} + \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sum_{2}^{\infty} \theta_i N_i \epsilon^{-\frac{\theta_i x}{D}}$$

when x is positive, and

$$\mathfrak{h} = C \cos \frac{\sigma_1 x}{D} + (C' - \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sigma_1 N_1) \sin \frac{\sigma_1 x}{D} + \frac{\frac{1}{2}A/D}{1 - D/b} \cdot \sum_{2}^{\infty} \theta_i N_i \epsilon^{-\frac{\theta_i x}{D}}$$

when x is negative

$$\left. \right\} \quad (46);$$

where C and C' denote arbitrary constants.

The motion represented by this solution, with any values of C and C′, is steady and stable throughout any finite length of the canal on each side of the ridge, provided the water is introduced at one end of the portion considered and taken away at the other conformably. If the canal extends to infinity in both directions, and if the water throughout be given in the state of motion corresponding to the solution (46); the motion throughout any finite distance on each side of the ridge will continue for an infinite time conformable to (46). The water, if given at rest, might be started into this state of motion in the following manner :—First displace its surface to the shape represented by equation (46), and apply a rigid corrugated lid to keep it exactly in this shape, so that it is now enclosed as it were in a rectangular tube with one side corrugated, two sides plane, and the fourth side (the bottom) plane, except at the place of the ridge. Next by means of a piston set the water gradually in motion in this tube. To begin with, the pressure on the lid will, in virtue of gravity, be non-uniform ; less at the high parts and greater at the low parts. If too great a velocity be given to the water by the piston the pressure will, in virtue of fluid motion, be greater at the high parts and less at the low parts. If the average velocity be made exactly U, the pressure will be uniform over the lid, which may then be dissolved ; thus the liquid is left moving steadily under the surface represented by equation (46) as free surface. But it is only in virtue of this motion being given to the fluid throughout an infinite length of the canal on each side of the ridge, that the motion can remain steady on each side of the ridge conformable to (46), except for the particular case of this general solution, corresponding to

$$C=0 \quad \text{and} \quad C'=\frac{\frac{1}{2}A/D}{1+D/b} \cdot \sigma_1 N_1 \quad . \quad . \quad . \quad (47),$$

which reduces (46) to

$$\mathfrak{h}=\frac{A/D}{1+D/b}\left(\sigma_1 N_1 \sin \frac{\sigma_1 x}{D} + \frac{1}{2}\sum_{2}^{\infty}\theta_i N_i \epsilon^{-\frac{\theta_i x}{D}}\right) \text{ when } x \text{ is positive} \left.\right\}$$

$$\text{and,} \qquad \mathfrak{h}=\frac{\frac{1}{2}A/D}{1+D/b}\sum_{2}^{\infty}\theta_i N_i \epsilon^{\frac{\theta_i x}{D}} \qquad \qquad \text{when } x \text{ is negative} \left.\right\} \quad (48);$$

this being the practical solution for the case of water flowing from the side of x negative over the single ridge and towards the side of x positive. It is the mathematical realization, for the case of a single ridge, of the circumstances described in Part I. No. 1 (*ante*, pp. 356–357), and is the mathematical

solution promised in the last sentence of
Part II. The demonstration that this is the
practically unique solution for inviscid
water flowing in a canal with a single
ridge, and the explanation of how any other
state of motion, such, for example, as that
represented by (46) with any value of C and
C', but given to the water throughout only
a finite distance on each side of the ridge,
settles into the permanent steady motion
represented by (48), must be reserved for
Part IV., which I hope will appear in the
January number.

Meantime the accompanying diagram
represents by two curves two cases of the
solution (46) for the particular value 2·456
of D/b ; that is to say, for velocity = ·6381
of the critical velocity $\sqrt{gD}$. The faint
curve represents the solution (39) (41), or,
which is the same, (46) with C = 0 and
C' = 0. The heavy curve represents the
practical solution (48). These curves were
drawn from calculations of a periodic
solution, according to the first of the two
methods indicated above, before I had
found the analytical solution (39) by which
the desired result could have been arrived
at with much less labour. The faint curve
was drawn first by direct calculation from
the periodic solution : the letters $\frac{1}{2} l$, $\frac{1}{4} l$,
$-\frac{1}{2} l$, $-\frac{1}{4} l$, &c., show on the two sides of
one ridge quarters of the distance from
ridge to ridge in the periodic solution, one
of the ridges being in the middle of the
diagram. The heavy curve is found by
adding to the ordinates of the faint curve
the ordinates of a curve of sines, found by
trial to as nearly as possible annul on the
one side, and to double on the other side,
the ordinates of the original curve. How
nearly perfect was the annulment on the
one side and the doubling on the other is
illustrated by the small-scale diagram an-
nexed (fig. 3), which has been drawn by
the engraver from a six times larger copy.
How nearly perfect the annulment and the
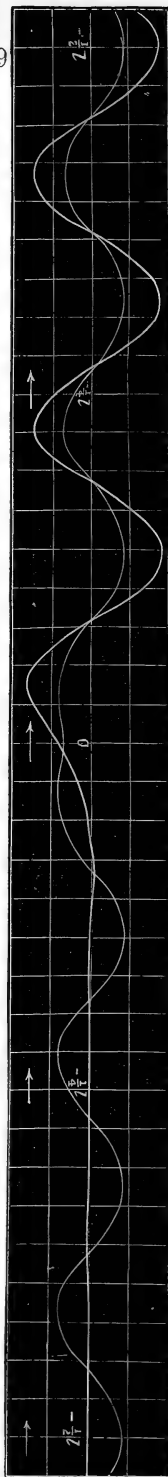
Fig. 3.

doubling ought to be at any particular distance from a single
ridge is now easily calculated from the second line of equation
(48), and will be actually calculated for the case of these
curves, and probably also for some other cases for numerical
illustrations, which I hope to give in Part IV.

LXI. *Decomposition of Glass by Carbon Dioxide held in
Solution in Capillary Films of Water.* By Prof. R.
BUNSEN [*].

IN an earlier publication [†] I have given my investigations
of the phenomena which present themselves when carbon
dioxide is allowed to act on capillary glass threads covered
with an extremely thin film of moisture.   According to these
investigations, it appears that 49·453 grammes of such capil-
lary threads are able in 109 days to take up so much carbon
dioxide, that on heating not less than 236·9 cubic centim. of
this gas is set free.   The gas so retained in the water-film,
showed towards pressure and temperature precisely the rela-
tions which are presented in the ordinary phenomena of gas
absorption by liquids.   In these experiments, as in all that
have been previously carried out, it has been assumed, both
from the result of direct observation and on theoretical
grounds, that the action of carbon dioxide on glass may be
entirely disregarded.   And, indeed, experiments were carried
out in my laboratory seventeen years ago by Dr. Emmerling,
which showed that glass vessels in which an 11 per cent.
solution of hydrochloric acid was boiled for hours together
did not lose 0·0005 grm. in weight.   If, in addition to this,
we bear in mind that under ordinary atmospheric pressure, at
15° C., water dissolves only 0·2 per cent. by weight of carbon
dioxide, an acid which is set free from all its compounds even
by the weakest acids, and, further, that repeated observations
show that dry carbon dioxide has practically no action upon
dry glass, then it must appear almost absurd to attempt to
explain the gradual fixation of carbon dioxide on glass dried
by calcium chloride by a chemical decomposition of the
glass.

But the matter presents itself under quite a different aspect
when we have regard to the phenomena of absorption as
occurring in capillary films.   Water which at 15° C. and

* Translated from Wied. *Ann.* x. pp. 161–165 (1886), by G. H. Bailey,
D.Sc., Ph.D.
† Wied. *Ann.* xxiv. p. 321 (1885).

0·76 metre pressure takes up about 0·2 per cent. of its weight of carbon dioxide behaves quite differently in capillary films, for it is not then under a pressure of one atmosphere, but under a very high capillary pressure, and so can take up so much more carbon dioxide, that, if we would study the decomposing action of the solution, we have no ground of comparison, and must solve the problem by direct experiment.

Such an experiment could not be carried out either before or during the previous experiments without destroying the capillary glass thread; and thus it was not possible to proceed with the examination of this question till the experiments already proceeding were finished.

The 49·453 grms. of glass used were, for this purpose, removed from the measuring tube and extracted with cold distilled water of such a purity that it left only $\frac{1}{500,000}$ solid residue on evaporation. For the extraction, portions of 300 grammes of water were taken, and the whole 3000 grammes so used were filtered through a double filter and evaporated to dryness in a platinum vessel. The residue dissolved in hydrochloric acid with evolution of carbon dioxide, and contained 0·8645 grm. of sodium chloride and 0·0608 grm. of silica along with unweighable traces of calcium chloride or potassium chloride.

From the composition of the capillary threads *, it appears therefore that there was not less than 2·882 grms. of the glass decomposed, or 5·83 per cent. of the whole quantity used. We see, then, that the chemical action of carbon dioxide under the influence of pressure in capillary films is far greater than we had any cause to expect. The carbon dioxide had, in the course of the experiments, taken up from the glass a quantity of soda corresponding to 0·7841 grm. sodium carbonate, and containing 0·325 grm. carbon dioxide.

Since sodium carbonate is not decomposed, even at very high temperatures, the 236·9 cubic centim. or 0·4659 grm. of carbon dioxide set free on heating could not arise from this decomposition product of the glass thread. But sodium carbonate takes up carbon dioxide and is transformed into the bicarbonate, and this carbon dioxide is set free again on heating, exactly in the same manner as in these observations.

It is thus to be determined whether the phenomena observed in capillary absorption can be exclusively attributed to the formation of sodium carbonate.

If we start from the most unfavourable supposition that all the sodium carbonate formed became bicarbonate, and that

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* Wied. *Ann.* xx. p. 545 (1883) [Phil. Mag. March 1884].

the temperature to which the glass was heated was sufficiently high to expel the whole of the carbon dioxide, then we can only account for 165·2 cubic centim. of carbon dioxide instead of 236·9 cubic centim.   There must therefore, even under the most unfavourable conditions, have been at least 71·7 cubic centim. of carbon dioxide fixed on the glass otherwise than by chemical union.   We cannot unfortunately determine how much, however, actually was due to the decomposition of the glass and how much to the capillary absorption.   If, then, carbon dioxide, under the conditions described, can overcome the affinity of silica for soda, a similar action, although in a lesser degree, may be expected from pure water.

That such an action really does take place may be expected, if one may draw a conclusion from the action at higher temperatures, as indicated by the following fact, which I had occasion to observe in the preliminary experiments on the estimation of the tension of water-vapour at very high temperatures. In these experiments I made use of narrow thick-walled tubes, sealed at the upper end, and attached at the lower end to a calibrated capillary tube 2 metres long, and which would withstand a pressure of 600 to 800 atmospheres.   In the wider part of the tube containing air there was, standing over the mercury column by which the approximate pressure was measured, a column of water, and this was heated to 550° C. in the thermostat described *.

At the part of the wall of the tube with which the water had been in contact there were alterations of a marked character.

The glass was transformed to more than a third of its thickness into a hard white porcelain-like mass, and the inner cavity of the tube diminished to one-tenth of its original diameter.   There can be no doubt therefore that glass and other silicates intended to be used in the examination of such questions are quite inapplicable.

In order to obtain trustworthy results in absolute measure without the interference of chemical influence, there remains scarcely any other course than to repeat the whole of the experiments on capillary absorption with very fine gold or platinum wire, and allow for the chemical action on the relatively small surface of the glass measuring-tube.

* Bunsen, *l. c.*

LXII. *Reply to the Observations made by* Messrs. T. E. Thorpe *and* A. W. Rücker *upon our Essay entitled "Intorno ad alcune formule date dal* Sig. Mendelejeff *e dai* Siggn. T. E. Thorpe *e* A. W. Rücker *per calcolare la temperatura critica della dilatazione termica."*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the course of some researches we were engaged upon two years ago we found it necessary to determine the critical temperature of all, or that of the greatest possible number of liquids. Inasmuch as this had only been arrived at experimentally in a comparatively small number of bodies, it occurred to us to apply to the remainder the theories and formula of Van der Waals, and thus calculate the temperature from the expansion caused by heat. We had already commenced our investigations when we became acquainted with the essay by Mendelejeff on the Expansion of Liquids published in the *Annales de Chimie et de Physique,* and with the summary of it in the essay by Mr. Thorpe and Mr. Rücker which appeared in the *Beiblätter,* entitled " On the Critical Temperatures of Bodies, and their Thermal Expansions as Liquids." As the formula deduced by Messrs. Thorpe and Rücker from the formula of Mendelejeff involved very simple calculations for determining the critical temperatures, we decided to adopt it.

Before doing so, however, we were desirous of establishing to our satisfaction the relative correctness of the formula, and the limits within which it could be legitimately applied. This preliminary inquiry gave rise to an essay entitled " Intorno ad alcune formule date dal Sig. D. Mendelejeff e dai Siggn. T. E. Thorpe e A. W. Rücker per calcolare la temperatura critica della dilatazione termica," published in the *Nuovo Cimento* for July, August, and September, 1884, pp. 91–104. This essay was also published in the *Gazzetta Chimica Italiana,* vol. xiv. 1884 ; and a report of it appeared shortly afterwards in the *Beiblätter zu den Annalen der Physik und Chemie,* and also in the *Journal de Physique de d'Almeida,* and, lastly, at the commencement of the present year, in the *Annales de Chimie et de Physique.* Messrs. Thorpe and Rücker, in a note inserted in the 'Philosophical Magazine' for May, have done us the honour to reply to our essay. We feel convinced that these gentlemen have only read the abridged report of our

paper published in the *Annales*, or they would not have waited two years before replying; and we believe also that had they read the paper in its entirety they would not have found it necessary to send any reply. We had no intention of criticising the work of Messrs. Thorpe and Rücker, but merely of showing by numerous examples the limits within which the formulas of Mendelejeff, and of Thorpe and Rücker, were applicable.

We believed that our inquiry would result in some utility, because it was not clear whether the formula of Mendelejeff, which was deduced from the comparison of the expansion of liquids, measured under a pressure of one atmosphere, could be applied to the expansion of liquids measured under a constant, or uniform, pressure, although *not* of one atmosphere.

We proved that the formula of Mendelejeff is valueless for showing the results of the experiments of Hirn, who has carefully studied the expansion of certain liquids under a constant pressure of 11 metres of mercury.

This fact appeared, and still does appear, to us to detract much from the general applicability of the formula of Mendelejeff for representing the expansion of liquids, which therefore we cannot consider otherwise than as empirical, and applicable only within narrow limits, but having with regard to the formulas more commonly adopted the merit of greater simplicity.

We were well aware that the formula of Mendelejeff and that of Messrs. Thorpe and Rücker (which is applicable within the same limits as that of Mendelejeff) were not applicable to water, and we especially called attention to the fact in the note at page 98 of our article, and also at page 102.

The formula of Messrs. Thorpe and Rücker for calculating the critical temperature leads to results which agree exactly with those obtained from experiments at temperatures lower than the normal boiling-point. We have already applied that formula for the calculation of critical temperatures of all liquids of which the thermal expansion has been studied.

We shall feel obliged by your kindly publishing the above statement.   Yours &c.,

A. Bartoli.
E. Stracciati.

LXIII. *Electromagnets.*—VI. *The Tension of Lines of Force.*
By R. H. M. BOSANQUET.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE experimental determination of the magnetic attrac-
tion corresponding to measured values of magnetic
induction has long been in my mind. It has some bearing
on my theory of permeability (see Phil. Mag. ser. 5, xix.
1885, p. 87 (note) and p. 94, in both of which places $\mathfrak{B}_2$ is
inadvertently printed for $\mathfrak{B}^2$); but its independent interest is
considerable.

After the following experiments were arranged, a paper on
a somewhat similar subject was read to the Royal Society by
Mr. Shelford Bidwell. From the accounts which have been
so far published, the method of this paper appears to be entirely

FIXED ELECTRO-MAGNET

SMALL COIL FOR MEASURING INDUCTIONS

MOVEABLE ELECTRO-MAGNET

COUNTERPOISE

SCALE PAN
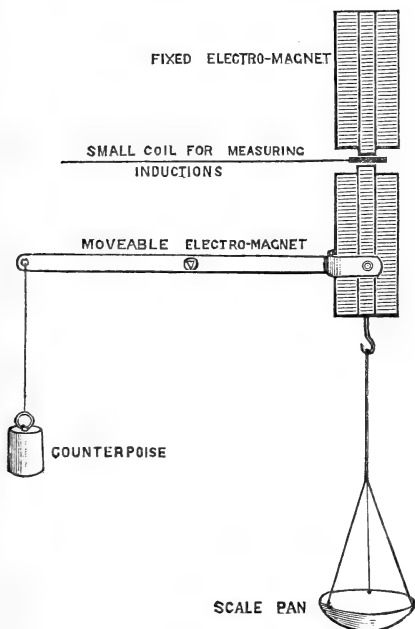
different from mine, as the magnetizing force and the weight
were alone directly measured: and in other respects the
method differs essentially from that which I have adopted.

The sketch represents the instrument constructed for these experiments. This consists of two cylindrical iron magnets, each 20 centim. long and ·526 centim. in diameter. Each is wound with 1096 coils of wire. One magnet is fixed in a wooden frame, while the other moves exactly under it smoothly and without friction, between two brass collars (not shown), through a vertical distance of about a quarter of an inch. The magnets were at first ground together so as to ensure a perfect contact. The lower or sliding magnet has attached to its lower end an iron hook, which passes through the table and sustains the wooden pan for the reception of the weights. To this magnet also is firmly attached a brass collar, in which are two holes for the pivots of the beam. The beam which carries the counterpoise is about 18 inches long ; it moves on knife-edges, and has at one end a half-round fork, which embraces freely the collar on the lower magnet, and is pivoted thereto. On the other end of the beam is hung a lead counterpoise exactly balancing the wooden pan and magnet.

The magnetizing current was sent through both electromagnets, while the induction was measured[*] by small coils placed round the point of contact of the two magnets. The inductions were measured by reversal. Weights were carefully added until the contact was broken, and the greatest weight sustained was taken as the observed number. This could be observed with certainty to the nearest ounce.

The first result is that the formula,

$$(W_1) \text{ weight in grammes} = \frac{S\mathfrak{B}^2}{8\pi.981} \text{ (S = section of contact)},$$

derived from (vol. ii.) Clerk Maxwell's ' Electricity and Magnetism,' p. 256, represents the values well on the whole ; but for very small inductions the weight sustained is several times as great as it should be according to this theory. And this did not arise from residual induction ; for when the magnetizing current was interrupted, no power was left to sustain any weight at all.

I therefore framed the empirical formula W , involving both $\mathfrak{B}$ and $\mathfrak{B}^2$; this represents the lower and middle values fairly, but not the highest.

It cannot, however, escape notice that the observed values are best satisfied by adding about 4 oz. to all weights calculated from W_1. Since, however, this addition must vanish for $\mathfrak{B}=0$, we cannot thus obtain a general formula. Note

* For the method see Phil. Mag. ser. 5, xix. p. 75.

that the induction has been forced up to the high value of 18,500. I have shown in recent papers that this quantity has no fixed limit in bar-electromagnets. This accords to some extent with the conclusions of Mr. Shelford Bidwell in the paper before referred to.

The table of experiments is followed by one containing the means of the errors of the formulæ W_1 and W_2 for the groups into which the experiments are divided.

Finally there are set out a few experiments made with the electromagnets not in contact, but separated to the distances mentioned by slips of card and wood. These experiments are not approximately satisfied by the foregoing formulæ ; but they would agree to some extent with W_1 if, instead of the coefficient

$$\frac{1}{8\pi \cdot 981} = \cdot 00004056,$$

we took about half that quantity, say $\cdot 000026$. The experiments at distance $\cdot 2$ would require a formula of the type of W_2. At present I am unable to give any explanation of these numbers.

The general truth of the law

$$\text{Tension} \propto S\mathfrak{B}^2$$

is sufficiently established.

In my theory of permeability I have assumed the tension within the magnetic body $\propto \mathfrak{B}$. It is clear that this assumption leads in that case to consequences which correspond with reality, and that the assumption tension $\propto S\mathfrak{B}^2$ does not do so in that case. I am at present unable to explain the divergence between the two points of view.

The weights have all been reduced to lbs. and ozs. to facilitate comparison with the actual experiments.

Batteries were used for weights up to about 5 lb. ; the dynamo from about $\ddot{z}$ lb. upwards.

Weights W_1 and W_2.

$$W_1 = \frac{S\mathfrak{B}^2}{8\pi \cdot 981}, \qquad \log S = \cdot 14671.$$

$$W_2 = S(\kappa\mathfrak{B}^2 + l\mathfrak{B}); \ \log \kappa = \overline{5}\cdot 55391; \ \log l = \overline{2}\cdot 76660.$$

Date.	$\mathfrak{B}$.	W obs.	W_1.	Diffs.	W_2.	Diffs.
		lb. oz.	lb. oz.	lb. oz.	lb. oz.	lb. oz.
July 17.	767	0 3	0 1·2	−0 1·8	0 3·3	+0 0·3
,, 20.	894	0 4	0 1·6	−0 2·4	0 4·0	0
,, 6.	800	0 5	0 1·3	−0 3·7	0 3·4	−0 1·6
,, 8.	800	0 5	0 1·3	−0 3·7	0 3·4	−0 1·6
,, 6.	825	0 6	0 1·4	−0 4·6	0 3·6	−0 2·4
,, 17.	1,534	0 6	0 4·7	−0 1·3	0 8·6	+0 2·6
,, 20.	1,845	0 8	0 6·8	−0 1·2	0 11·4	+0 3·4
,, 8.	1,663	0 10	0 5·6	−0 4·4	0 9·7	−0 0·3
,, 17.	2,544	1 0	0 13·0	−0 3·0	1 3·0	+0 3·0
,, 8.	2,818	1 6	1 0	−0 6	1 6	0
,, 20.	3,180	1 6	1 4	−0 2	1 11	+0 5
,, 8.	3,113	1 9	1 3	−0 6	1 10	+0 1
,, 17.	3,607	1 12	1 10	−0 2	2 1	+0 5
,, 17.	4,549	2 13	2 9	−0 4	3 2	+0 5
,, 8.	5,000	3 6	3 2	−0 4	3 11	+0 5
,, 17.	5,148	3 9	3 5	−0 4	3 15	+0 6
,, 17.	5,222	3 13	3 7	−0 6	4 4	+0 7
,, 20.	5,205	3 13	3 6	−0 7	3 15	+0 2
,, 2.	5,588	4 4	3 15	−0 5	4 7	+0 3
,, 20.	7,047	5 11	6 4	+0 9	6 12	+1 1
,, 22.	6,630	5 11	5 8	−0 3	6 1	+0 6
,, 8.	6,759	5 12	5 12	0	6 4	+0 8
,, 2.	6,516	5 13	5 5	−0 8	5 13	0
,, 5.	6,841	6 1	5 14	−0 3	6 7	+0 6
,, 2.	6,594	6 2	5 7	−0 11	6 0	−0 2
,, 23.	7,110	6 8	6 5	−0 3	4 9	−1 15
,, 23.	7,464	6 10	6 15	+0 5	7 8	+0 14
,, 2.	7,209	6 12	6 13	+0 1	7 1	+0 5
,, 9.	6,977	6 12	6 2	−0 10	6 10	−0 2
,, 19.	6,645	7 1	5 9	−1 8	6 1	−1 0
,, 23.	7,510	7 1	7 1	0	7 9	+0 8
,, 23.	7,414	7 2	6 14	−0 4	7 7	+0 5
,, 20.	8,360	8 8	8 12	+0 4	9 4	+0 12
,, 20.	9,195	10 0	10 10	+0 10	11 0	+1 0
,, 9.	8,636	10 2	9 6	−0 12	9 13	−0 5
,, 9.	9,059	11 7	10 5	−1 2	10 11	−0 12
,, 5.	11,235	15 13	15 13	0	16 0	+0 3
,, 19.	12,237	21 2	18 12	−2 6	18 12	−2 6
,, 22.	12,587	21 3	19 14	−1 5	19 13	−1 6
,, 5.	14,211	26 7	25 7	−1 0	24 15	−1 8
,, 19.	14,342	28 2	25 13	−2 5	25 6	−2 12
,, 22.	15,429	30 12	29 13	−0 15	29 2	−1 10
,, 19.	16,015	34 5	32 2	−2 3	31 4	−3 1
,, 5.	16,384	35 1	33 11	−1 6	32 11	−2 6
,, 22.	16,433	35 4	33 14	−1 6	32 14	−2 6
,, 19.	16,387	35 15	33 11	−2 4	32 11	−3 4
,, 2.	17,302	36 5	37 8	+1 3	36 4	−0 1
,, 2.	17,876	36 9	40 1	+3 8	38 9	+2 0
,, 2.	17,731	36 14	39 6	+2 8	38 0	+1 2
,, 22.	17,292	36 13	37 8	+0 11	36 3	−0 10
,, 2.	17,554	37 3	38 10	+1 7	37 4	+0 1
,, 19.	17,379	41 3	37 14	−3 5	36 9	−4 10
,, 22.	18,582	41 9	43 5	+1 12	41 9	0
,, 5.	17,596	41 12	38 13	−2 15	37 7	−4 5
,, 22.	19,519	44 3	47 12	+3 9	42 11	−1 8
,, 19.	17,905	44 5	40 3	−4 2	38 11	−5 10
,, 5.	18,497	45 0	43 13	−1 3	41 3	−3 13

W obs.		Arithmetic mean of errors of W_1 and W_2.		Number of experiments.
		$W_{1}.$	$W_{3}.$	
	lb. oz.	lb. oz.	lb. oz.	
From to	0 3 1 6	-0 3·21	0 0·34	10
From to	1 6 5 11	-0 3·1	$+0$ 5·6	10
From to	5 11 7 1	-0 5·6	-0 1·2	10
From to	7 1 26 7	-0 9·5	-0 5·7	10
From to	26 7 36 13	-0 4·1	-1 4·8	10
From to	36 13 45 0	-0 11·0	-2 13·2	7

Weights sustained by Magnets when separated.

$$\kappa = \frac{W}{S\mathfrak{B}^2}.$$

Date.	W obs.	$\mathfrak{B}$.	κ.	Distance of magnets apart.	Mean κ.
	lb. oz.				
1886. August 2nd.	2 0	4803·5	·00002805	in. ·028	·00002638
	5 0	8022·1	·00002514		
	10 12	12667	·00002168		
	16 4	13893	·00002724		
	17 14	14588	·00002718		
	19 2	14858	·00002803		
	25 13	17464	·00002738		
3rd & 4th.	1 5	3736·4	·00003042	·060	·00002584
	3 5	6330·2	·00002675		
	7 5	9709·6	·00002510		
	10 9	11743	·00002478		
	12 5	12449	·00002571		
	14 5	13878	·00002404		
	17 11	15415	·00002408		
4th & 5th.	0 7	1611·8	·00005449	·2	
	0 10	2545·1	·00003122		
	1 5	4371·3	·00002222		
	2 5	6095·6	·00002014		
	2 14	6909·7	·00001948		
	3 12	8034·0	·00001880		
	5 9	10437	·00001652		

LXIV. *Intelligence and Miscellaneous Articles.*

SILK *v.* WIRE, OR THE "GHOST" IN THE GALVANOMETER.
BY R. H. M. BOSANQUET.

THE ballistic galvanometer which I used for some years for the measurement of induction currents consisted of a small astatic pair of needles with mirror, surrounded by a small coil of very low resistance. The suspension was from a silk fibre about six inches long, the fibre being left just stout enough to carry the weight. This whole combination was extremely sensitive and for the most part convenient to work with.

The silk suspension, however, has certain troublesome properties. I shall not here enter into the manner in which it gradually untwists itself when stretched, or into its property of taking a set from any change of position; but shall confine myself to the appearance which we called the "ghost."

At certain times the needles of the galvanometer would move about with sudden and capricious movements, the mirror often traversing several degrees of the scale. The decision and sharpness of the movements were very remarkable, and we habitually spoke of their cause as the "ghost."

The ghost used to visit us mostly in summer between the hours of nine and eleven in the forenoon, and about six in the evening *. When these movements began it was no use attempting to work with the galvanometer. There can be no doubt that the movements were due to the solar heat falling more or less directly on the instrument and causing hygroscopic changes in the silk fibre.

In the early summer of this year I found it necessary to free myself from this source of interruption, and constructed a galvanometer with a wire suspension. The difficulty consists in combining a needle system large enough to vibrate very slowly on the wire suspension, with a coil having sufficient power over the needles, and at the same time a low enough resistance.

The needles are stout knitting-needles seven inches long. They are hung from a support fastened to the wall by a very fine wire about 5 feet long. The needles are very nearly astatic, and the complete double vibration takes a little over half a minute. The coil consists of about 500 turns of No. 20 B. W. G. The resistance is much greater than that of the old instrument and the loss of sensitiveness is an inconvenience; but the instrument works well in connection with our large earth induction-coil of 250 turns of the same wire, and it is entirely free from the visits of the ghost.

It is my conviction that silk and thread suspensions are sources of error and inconvenience to an extent that has been imperfectly realized; and that they ought to be entirely banished from all instruments of precision.

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* The aspect of the galvanometer-room is north and east.

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END OF THE TWENTY-SECOND VOLUME.

Fig. 2.

Soft iron temporarily magnetized longitudinally

Fig. 3

Iron wire repeatedly longitudinally magnetized temporarily

Fig. 4.

Nickel temporarily magnetized longitudinally

Fig. 5.

Soft iron permanently magnetized longitudinally

Fig. 6.

Nickel permanently magnetized longitudinally

Fig. 7.

Iron temporarily magnetized transversely

Fig. 8.

Nickel temporarily magnetized transversely

Fig. 9.

Iron permanently magnetized transversely

Fig. 1

Fig. 10

Fig. 11.

Fig 1
⅓rd full size.

Fig 4.

Line of no current

Fig 2.
2 full size

Fig. 3.

Fig. 1.

178°C.

100°C.

SOLAR
ENERGY.

0°C.

Normal Spectrum.

Fig. 2.

Bolometer and Protecting Case.

Fig. 1.

Fig. 2.

Fig. 3.

Apparatus for determining Relation between Index of Refraction
and Wave-length.

$n = \phi(\lambda)$ for Rock-salt Prism.

Étude des bandes telluriques α, B et A du spectre solaire,
par M. A. Cornu.

Bande α

Bande B

Bande A

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,
22 ALBEMARLE STREET, LONDON, W.

The next ANNUAL GENERAL MEETING will be held at BIRMINGHAM, commencing on WEDNESDAY, SEPTEMBER 1.

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Principal of McGill College, Montreal, Canada.

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LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE,

AND

JOURNAL OF SCIENCE.

Being a Continuation of Tilloch's 'Philosophical Magazine,'
Nicholson's 'Journal,' and Thomson's 'Annals of Philosophy.'

FIFTH SERIES.

Nº 137.—OCTOBER 1886.